This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

- *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.
- *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.
- *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.
- *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

LANE MEDICAL LIBRARY STANFORD
A175 .V46 1876 STOR
The logic of chance : An essay on the fo

24503385064

Venn, says Pearl: Med. Biometry, ed. 2, 1930 p. 313,
Suffers 'from a curiously diffuse and wandering style, but sound as to doctrine." Pearl recommends Venn, in connection with the theory of probability.

H. Gray
February 1933.

THE
LOGIC OF CHANCE.

ed. 1 = 1866 or '7

1876

= 1 [illegible]

THE

LOGIC OF CHANCE

AN ESSAY

ON THE FOUNDATIONS AND PROVINCE OF
THE THEORY OF PROBABILITY,

WITH ESPECIAL REFERENCE TO ITS LOGICAL BEARINGS
AND ITS APPLICATION TO

MORAL AND SOCIAL SCIENCE.

BY

JOHN VENN, M.A.
FELLOW AND LECTURER IN THE MORAL SCIENCES, GONVILLE AND CAIUS COLLEGE,
CAMBRIDGE.
EXAMINER IN LOGIC AND MORAL PHILOSOPHY IN THE
UNIVERSITY OF LONDON.

> "So careful of the type she seems
> So careless of the single life."

SECOND EDITION, RE-WRITTEN AND GREATLY ENLARGED.

London:
MACMILLAN AND CO.
1876.

[*All Rights reserved.*]

47

Cambridge:
PRINTED BY C. J. CLAY, M.A.
AT THE UNIVERSITY PRESS.

VA6
1876

PREFACE TO FIRST EDITION[1].

ANY work on Probability by a Cambridge man will be so likely to have its scope and its general treatment of the subject prejudged, that it may be well to state at the outset that the following Essay is in no sense mathematical. Not only, to quote a common but often delusive assurance, will 'no knowledge of mathematics beyond the simple rules of Arithmetic' be required to understand these pages, but it is not intended that any such knowledge should be acquired by the process of reading them. Of the two or three occasions on which algebraical formulæ occur they will not be found to form any essential part of the text.

The science of Probability occupies at present a somewhat anomalous position. It is impossible, I think, not to observe in it some of the marks and consequent disadvantages of a *sectional* study. By a small body of ardent students it has been cultivated with great assiduity, and the results they have obtained will always be reckoned among the most extraordinary products of mathematical genius. But by the general body of thinking men its principles seem to be regarded with indifference or suspicion. Such persons may admire the ingenuity displayed, and be struck with the profundity of many of the calculations, but there seems to

---

[1] *With trifling omissions and alterations.*

80724

them, if I may so express it, an *unreality* about the whole treatment of the subject. To many persons the mention of Probability suggests little else than the notion of a set of rules, very ingenious and profound rules no doubt, with which mathematicians amuse themselves by setting and solving puzzles.

It must be admitted that some ground has been given for such an opinion. The examples commonly selected by writers on the subject, though very well adapted to illustrate its rules, are for the most part of a special and peculiar character, such as those relating to dice and cards. When they have searched for illustrations drawn from the practical business of life, they have very generally, but unfortunately, hit upon just the sort of instances which, as I shall endeavour to show hereafter, are among the very worst that could be chosen for the purpose. It is scarcely possible for any unprejudiced person to read what has been written about the credibility of witnesses by eminent writers, without his experiencing an invincible distrust of the principles which they adopt. To say that the rules of evidence sometimes given by such writers are broken in practice, would scarcely be correct; for the rules are of such a kind as generally to defy any attempt to appeal to them in practice.

This supposed want of harmony between Probability and other branches of Philosophy is perfectly erroneous. It arises from the belief that Probability is a branch of mathematics trying to intrude itself on to ground which does not altogether belong to it. I shall endeavour to show that this belief is unfounded. To answer correctly the sort of questions to which the science introduces us does generally demand some knowledge of mathematics, often a great knowledge, but the discussion of the fundamental principles on which the rules are based does not necessarily require any such qualification. Questions might arise in other sciences, in Geology, for example, which could only be answered by the aid of arithmetical calculations. In such a case any one would admit that the arithmetic was extraneous and accidental. However many questions of this kind there might be here, those persons who do not care to work out special results for themselves might still have an accurate knowledge of the principles of the science, and even considerable acquaintance with the details of it. The same holds true in Probability; its connection with mathematics, though certainly far closer than that of most other sciences, is still of much the same kind. It is principally when we wish to work out results for ourselves that mathematical knowledge is required; without such knowledge the student may still have a firm grasp of the principles and even see his way to many of the derivative results.

The opinion that Probability, instead of being a branch of the general science of evidence which happens to make much use of mathematics, *is* a portion of mathematics, erroneous as it is, has yet been very disadvantageous to the science in several ways. Students of Philosophy in general have thence conceived a prejudice against Probability, which has for the most part deterred them from examining it. As soon as a subject comes to be considered 'mathematical' its claims seem generally, by the mass of readers, to be either on the one hand scouted or at least courteously rejected, or on the other to be blindly accepted with all their assumed consequences. Of impartial and liberal criticism it obtains little or nothing.

The consequences of this state of things have been, I think, disastrous to the students themselves of Probability. No science can safely be abandoned entirely to its own devotees. Its details of course can only be studied by those who [illegible] of their special application. But its general principles are sure to be neglected if it is not exposed continually to the free criticism of those whose main culture has been of a more general character. Probability has been very much abandoned to mathematicians, who as mathematicians have generally been unwilling to treat it thoroughly. They have worked out its results, it is true, with wonderful acuteness, and the greatest ingenuity has been shown in solving various problems that arose, and deducing subordinate rules. And this was all that they could in fairness be expected to do. Any subject which has been discussed by such men as Laplace and Poisson, and on which they have exhausted all their powers of analysis, could not fail to be profoundly treated, so far as it fell within their province. But from this province the real principles of the science have generally been excluded, or so meagrely discussed that they had better have been omitted altogether. Treating the subject as mathematicians such writers have naturally taken it up at the point where their mathematics would best come into play, and that of course has not been at the foundations. In the works of most writers upon the subject we should search in vain for anything like a critical discussion of the fundamental principles upon which its rules rest, the class of enquiries to which it is most properly applicable, or the relation it bears to Logic and the general rules of inductive evidence.

This want of precision as to ultimate principles is perfectly compatible here, as it is in the departments of Morals and Politics, with a general agreement on processes and results. But it is, to say the least, unphilosophical, and dangerous a state of things in which positive error is always liable to arise whenever the process of controversy forces us to appeal to the foundations of the science.

With regard to the remarks in the last few paragraphs, prominent exceptions must be made in the case of two recent works at least[1]. The first of these is Professor De Morgan's *Formal Logic.* He has there given an investigation into the foundations of Probability as conceived by him, and nothing can be more complete and precise than his statement of principles, and his deductions from them. If I could at all agree with these principles there would have been no necessity for the following essay, as I could not hope to add anything to their foundation, and should be far indeed from rivalling his lucid statement of them. But in his scheme Probability is regarded very much from the Conceptualist point of view; as stated in the preface, he considers that Probability is concerned with formal inferences in which the premises are entertained with a conviction short of absolute certainty. With this view I cannot agree. As I have entered into criticism of some points of his scheme in one of the following chapters, and shall have occasion frequently to refer to his work, I need say no more about it here. The other work to which I refer is the profound *Laws of Thought* of the late Professor Boole, to which somewhat similar remarks may in part be applied. Owing however to his peculiar treatment of the subject, I have scarcely anywhere come into contact with any of his expressed opinions.

The view of the province of Probability adopted in this Essay differs so radically from that of most other writers on the subject, and especially from that of those just referred to, that I have thought it better, as regards details, to avoid all criticism of the opinions of others, except where conflict was unavoidable. With regard to that radical difference itself Bacon's remark applies, behind which I must shelter myself from any charge of presumption,—"Quod ad universalem istam reprehensionem attinet, certissimum vere est rem reputanti, eam et magis probabilem esse et magis modestam, quam si facta fuisset ex parte."

---

[1] I am here speaking, of course, of those only who have expressly treated of the foundations of the science. Mr Todhunter's admirable work on the *History of the Theory of Probability* being, as the name denotes, mainly historical, such enquiries have not directly fallen within his province.

Almost the only writer who seems to me to have expressed a just view of the nature and foundation of the rules of Probability is Mr Mill, in his *System of Logic*[1]. His treatment of the subject is however very brief, and a considerable portion of the space which he has devoted to it is occupied by the discussion of one or two special examples. There are moreover some errors, as it seems to me, in what he has written, which will be referred to in some of the following chapters.

The reference to the work just mentioned will serve to convey a general idea of the view of Probability adopted in this Essay. With what may be called the Material view of Logic as opposed to the Formal or Conceptualist,—with that which regards it as taking cognisance of laws of things and not of the laws of our own minds in thinking about things,—I am in entire accordance. Of the province of Logic, regarded from this point of view, and under its widest aspect, Probability may, in my opinion, be considered to be a portion. The principal objects of this Essay are to ascertain how great a portion it comprises, where we are to draw the boundary between it and the contiguous branches of the general science of evidence, what are the ultimate foundations upon which its rules rest, what the nature of the evidence they are capable of affording, and to what class of subjects they may most fitly be applied. That the science of Probability, on this view of it, contains something more important than the results of a system of mathematical assumptions, is obvious. I am convinced moreover that it can and ought to be rendered both interesting and intelligible to ordinary readers who have any taste for philosophy. In other words, if the large and growing body of readers who can find pleasure in the study of books like Mill's *Logic* and Whewell's *Inductive Sciences*, turn with aversion from a work on Probability, the cause in the latter case must lie either in the view of the subject or in the manner and style of the book.

---

[1] This remark, and that at the commencement of the last paragraph, having been misunderstood, I ought to say that the only sense in which originality is claimed for this Essay is in the thorough working out of the Material view of Logic as applied to Probability. I have given a pretty full discussion of the general principles of this view in the tenth chapter, and have there pointed out some of the peculiarities to which it leads.

I take this opportunity of thanking several friends, amongst whom I must especially mention Mr Todhunter, of St John's College, and Mr H. Sidgwick, of Trinity College, for the trouble they have kindly taken in looking over the proof-sheets, whilst this work was passing through the press. To the former in particular my thanks are due for thus adding to the obligations which I, as an old pupil, already owed him, by taking an amount of trouble, in making suggestions and corrections for the benefit of another, which few would care to take for anything but a work of their own. His extensive knowledge of the subject, and his extremely accurate judgment, render the service he has thus afforded me of the greatest possible value.

GONVILLE AND CAIUS COLLEGE,
September, 1866,

PREFACE TO SECOND EDITION.

THE principal reason for designating this volume a second edition consists in the fact that the greater portion of what may be termed the first edition is incorporated into it. Besides various omissions (principally where the former treatment has since seemed to me needlessly prolix), I have added new matter, not much inferior in amount to the whole of the original work. In addition, moreover, to these alterations in the matter, the general arrangement of the subject as regards the successive chapters has been completely changed; the former arrangement having been (as it now seems to me) justly objected to as deficient and awkward in method.

After saying this, it ought to be explained whether any change of general view or results will be found in the present treatment.

The general view of Probability adopted is quite unchanged, further reading and reflection having only confirmed me in the conviction that this is the soundest and most fruitful way of regarding the subject. It is the more necessary to say this, as to a cursory reader it might seem otherwise; owing to my having endeavoured to avoid the needlessly polemical tone which, as is often the case with those who are making their first essay in writing upon any subject, was doubtless too prominent in the former edition. I have not thought it necessary, of course, except in one or two cases, to indicate points of detail which it has seemed necessary to correct.

A number of new discussions have been introduced upon topics which were but little or not at all treated before. The principal of these refer to the nature and physical origin of Laws of Error (Ch. II.); the general view of Logic, and consequently of Probability, termed the Material view, adopted here (Ch. X.); a brief history and criticism of the various opinions held on the subject of Modality (Ch. XII.); the logical principles underlying the method of Least Squares (Ch. XIII.); and the practices of Insurance and Gambling, so far as the principles involved in them are concerned (Ch. XV.). The Chapter on the Credibility of Extraordinary Stories is also mainly new; this was the portion of the former work which has since seemed to me the least satisfactory, but owing to the extreme intricacy of the subject I am far from feeling thoroughly satisfied with it even now.

I have again to thank several friends for the assistance they have so kindly afforded. Amongst these I must prominently mention Mr C. J. Monro, late fellow of Trinity. It is only the truth to say that I have derived more assistance from his suggestions and criticisms than has been consciously obtained from all other external sources together. Much of this criticism has been given privately in letters, and notes on the proof-sheets; but one of the most elaborate of his discussions of the subject was communicated to the Cambridge Philosophical Society some years ago; as it was not published, however, I am unfortunately unable to refer the reader to it. I ought to add that he is not in any way committed to any of my opinions upon the subject, from some of which in fact he more or less dissents. I am also much indebted to Mr J. W. L. Glaisher, also of Trinity College, for many hints and references to various publications upon the subject of Least Squares, and for careful criticism (given in the midst of much other labour) of the chapter in which that subject is treated of.

I need not add that, like every one else who has had to discuss the subject of Probability during the last ten years, I have made constant use of Mr Todhunter's History.

I may take this opportunity of adding that a considerable portion of the tenth chapter has recently appeared in the January number of *Mind*, and that the substance of several chapters, especially in the more logical parts, has formed part of my ordinary lectures in Cambridge; the foundation and logical treatment of Probability being now expressly included in the Schedule of Subjects for the Moral Sciences Tripos.

March, 1876.

TABLE OF CONTENTS.

PART I.

PHYSICAL FOUNDATIONS OF THE SCIENCE OF PROBABILITY.

CHH. I—IV.

CHAPTER I.

THE SERIES OF PROBABILITY.

§§ 1, 2. Distinction between the proportional propositions of Probability, and the propositions of Logic.

3, 4. The former are best regarded as presenting a *series* of individuals,

5. Which may occur in any order of time,

6, 7. And which present themselves in groups.

8. Comparison of the above with the ordinary phraseology.

9, 10. These series ultimately fluctuate,

11. Especially in the case of moral and social phenomena,

12. Though in the case of games of chance the fluctuation is utterly inappreciable.

13, 14. In this latter case only can rigorous inferences be drawn.

15. The Petersburg Problem.

CHAPTER II.

ARRANGEMENT AND FORMATION OF THE SERIES. LAWS OF ERROR.

§§ 1, 2. Indication of the nature of a Law of Error or Divergence.

3. Is there necessarily but one such law,

4. Applicable to widely distinct classes of things?

§§ 5, 6. This cannot be proved directly by statistics;
7, 8. Nor deductively;
9. Nor by the Method of Least Squares.
10. Distinction between Laws of Error and the Method of Least Squares.
11. Supposed existence of types.
12—14. Homogeneous and heterogeneous classes.
15. The type in the case of human stature, &c.
16. The type in méntal characteristics.
17, 18. Applications of the foregoing principles and results.

CHAPTER III.

ORIGIN OR PROCESS OF CAUSATION OF THE SERIES.

§ 1. The causes consist of (1) objects,
2, 3. Which may or may not be distinguishable into natural kinds,
4—6. And (2) agencies.
7. Requisites demanded in the above:
8, 9. Consequences of their absence.
10. Where are the required causes found?
11, 12. Not in the direct results of human will.
13—15. Examination of apparent exceptions.
16, 17. Meaning of Randomness.

CHAPTER IV.

HOW TO DISCOVER AND PROVE THE SERIES.

§ 1. The data of Probability are established by experience;
2. Though in practice problems are solved deductively.
3—7. Mechanical instance to show the inadequacy of any à priori proof.
8. The Principle of Sufficient Reason inapplicable.

§ 9. Evidence of actual experience.
10, 11. Further examination of the causes.
12, 13. Distinction between the succession of physical events and the Doctrine of Combinations.
14, 15. Remarks of Laplace on this subject.
16. Bernoulli's Theorem;
17, 18. Its inapplicability to social phenomena.
19. Summation of preceding results.
20. On Averages:
21, 22. Distinction between their physical and mathematical aspects.
23, 24. No distinction between means and averages.
25. Purposes for which averages are taken.
26, 27. Mean and Probable Error.

PART II.

LOGICAL SUPERSTRUCTURE ON THE ABOVE PHYSICAL FOUNDATIONS. CHH. V—XIV.

CHAPTER V.

GRADATIONS OF BELIEF.

§§ 1, 2. Preliminary remarks.
3, 4. Are we accurately conscious of gradations of belief?
5. Probability only concerned with part of this enquiry.
6. Difficulty of measuring our belief;
7. Owing to intrusion of emotions,
8. And complexity of the evidence.
9. And when measured, is it always correct?
10, 11. Distinction between logical and psychological views.
12—16. Analogy of Formal Logic fails to show that we can thus detach and measure our belief.
17. Apparent evidence of popular language to the contrary.

§ 18. How is *full* belief justified in inductive enquiry?
19—23. Attempt to show how *partial* belief may be similarly justified.
24—28. Extension of this explanation to cases which cannot be repeated in experience.
29. Can other emotions besides belief be thus quantified?
30. Errors thus connected with the Petersburg Problem.
31, 32. The emotion of surprise is a partial exception.
33. Objective and subjective phraseology.
34. Both may be needed.
35. Definition of probability,
36. Introduces the notion of a 'limit',
37. And implies, vaguely, some degree of belief.

CHAPTER VI.

THE RULES OF INFERENCE IN PROBABILITY.

§ 1. Nature of these inferences.
2. Inferences by addition and subtraction.
3. Inferences by multiplication and division.
4—6. Rule for independent events.
7. Other rules sometimes introduced.
8. All rules may be interpreted in terms of belief.
9—11. Rules of so-called Inverse Probability.
12, 13. Nature of the assumption involved in them:
14—17. Arbitrary character of this assumption.

CHAPTER VII.

THE RULE OF SUCCESSION.

§ 1. Reasons for desiring some such rule:
2. Though it can scarcely belong to Probability.
3. Distinction between Probability and Induction.

§§ 4, 5. Impossibility of reducing the various rules of the latter under one head.
6. Statement of the Rule of Succession;
7. It must not be regarded as a mere instinct;
8, 9. Its incorrectness and inapplicability;
10, 11. Other interpretations of it.

CHAPTER VIII.

INDUCTION.

§§ 1—5. Statement of the Inductive problem, and origin of the Inductive inference.
6. Relation of Probability to Induction.
7—9. The two are sometimes merged into one.
10. Extent to which causation is needed in Probability.
11—13. Difficulty of referring an individual to a class:
14. This difficulty but slight in Logic,
15, 16. But leads to perplexity in Probability:
17—21. Mild form of this perplexity;
22, 23. Serious form.
24—27. Application to Life Insurance.
28, 29. Meaning of 'the value of a life'.
30, 31. Successive specialization of the classes to which objects are referred.
32. Summary of results.
33, 34. Mill's view of Induction.
35. Whewell's view.
36, 37. Extreme complexity of the aggregate inductive evidence of any fact.
38. Bearing of the above upon Rules of Discovery.

CHAPTER IX.

CAUSATION AND DESIGN.

§§ 1, 2. Statement of the common scientific view.
3. The cause is (1) the sum-total of antecedents,
4. And (2) these antecedents are immediate.
5. No 'chain' of causation.
6. Origin of popular phraseology.
7, 8. Plurality of Causes and Effects.
9, 10. The common view needs extension.
11. The conception of Uniformity.
12, 13. Bearing of causation and uniformity upon the foundations of Probability.
14, 15. Causation is not demanded.
16. Application of statistics to the question of free-will.
17—20. Defects in the common arguments on this point.
21. Chance as an agent.
22. Laplace on Bernoulli's theorem.
23. Reference to Arbuthnott's argument.
24. Providence and Fatalism.
25—27. Detection of design by observation of results.
28, 29. Random or designed distribution of stars.

CHAPTER X.

MATERIAL AND FORMAL LOGIC.

§§ 1, 2. Broad distinction between these views;
3. Origin of this distinction.
4—7. Characteristics of Conceptualist Logic.
8. Formal and Conceptual nearly synonymous.
9, 10. Characteristics of Material Logic.

§§ 11—14. Merits and deficiencies of each view.
15. Can the distinction between Formal and Conceptualist Logic be uniformly maintained?
16, 17. Facts and conceptions imperfectly contrasted.
18, 19. Starting point of the Material Logician.
20—22. Doubtful stage of facts only occasional in Inductive Logic,
23—25. But normal in Probability.
26—28. Consequent difficulty of avoiding Conceptualist phraseology.

CHAPTER XI.

CONSEQUENCES OF THE FOREGOING DISTINCTIONS.

§§ 1, 2. Probability has no relation to time.
3, 4. Butler and Mill on Probability before and after the event.
5. Other attempts at explaining the difficulty.
6—8. What is really meant by the distinction.
9. Origin of the common mistake.
10—12. Examples in illustration of this view.
13. Is Probability relative?
14. What is really meant by this expression.
15. Objections to terming Probability relative.
16, 17. In suitable examples the difficulty scarcely presents itself.

CHAPTER XII.

ON MODALITY.

§ 1. Various senses of Modality;
2. Having mostly some relation to Probability.
3. Modality must be recognized.
4. Sometimes relegated to the predicate,
5, 6. Sometimes incorrectly rejected altogether.

§§ 7, 8. Common practical recognition of it.
9—11. Modal propositions in Logic and in Probability.
12. Aristotelian view of the Modals;
13, 14. Founded on extinct philosophical views;
15. But long and widely maintained.
16. Kant's general view.
17—19. The number of modal divisions admitted by various logicians.
20. Influence of the theory of Probability.
21, 22. Modal syllogisms.
23. Popular modal phraseology.
24—26. Probable and Dialectic syllogisms.
27, 28. Modal difficulties occur in Jurisprudence.
29, 30. Proposed standards of legal certainty.
31. Rejected formally in English Law, but possibly recognized practically.
32. How, if so, it might be determined.

CHAPTER XIII.

METHOD OF LEAST SQUARES, OR ADVANTAGE OF TAKING A MEAN.

§§ 1—4. General statement of the object desired in the use of such methods.
5, 6. No method is certainly accurate:
7—10. But any reasonable method is good:
11. Nature of the superiority of one over the other.
12. Combination and weighting of observations.
13. Various modes of combination.
14, 15. Laws of Error and the Method of Least Squares.
16, 17. Comparative inferiority of single observations.
18—23. Detailed examination of a numerical example.

CHAPTER XIV.

FALLACIES.

§§ 1—4. (I.) Errors in judging of events after they have happened.
5—9. Very various judgments may be thus meant.
10—12. (II.) Undue limitation of the notion of Probability.
13—19. (III.) Double or Quits: the Martingale.
20—23. (IV.) Inadequate realization of large numbers.
24—26. Production of works of art by chance.
27. Arguments for heredity.
28—33. (V.) Confusion between Probability and Induction.
34—36. (VI.) Undue neglect of small chances.
37. (VII.) Judging by the event.

PART III.

VARIOUS APPLICATIONS OF THE THEORY OF PROBABILITY, Chh. XV—XVIII.

CHAPTER XV.

INSURANCE AND GAMBLING.

§§ 1, 2. The certainties and uncertainties of life.
3—5. Insurance a means of diminishing the uncertainties.
6, 7. Gambling a means of increasing them.
8, 9. Various forms of Gambling.
10, 11. Psychological assumptions about these practices.
12—14. Doubtful nature of these assumptions.
15—17. Is gambling necessarily disadvantageous?

CHAPTER XVI.

APPLICATION OF PROBABILITY TO TESTIMONY.

§§ 1, 2. Doubtful applicability of Probability to testimony.
3, 4. Conditions of such applicability.
5. Reasons for the above conditions.
6, 7. Are these conditions fulfilled in the case of testimony?
8. The appeal here is not directly to statistics.
9, 10. Illustrations of the above.
11. Is any application of Probability to testimony valid?

CHAPTER XVII.

CREDIBILITY OF EXTRAORDINARY STORIES.

§ 1. Improbability before and after the event.
2, 3. Does the rejection of this lead to the conclusion that the credibility of a story is independent of its nature?
4. General and special credibility of a witness.
5--9. Distinction between alternative and open questions, and the usual rules for application of testimony to each.
10, 11. Testimony of worthless witnesses.
12 -14. Common practical ways of regarding such problems.
15. Extraordinary stories not necessarily less probable.
16 -19. Meaning of the term extraordinary, and its distinction from miraculous.
20, 21. Combination of testimony.
22, 23. Scientific meaning of a miracle.
24—26. Two distinct prepossessions in regard to miracles, and their logical consequences.
27. Difficulty of discussing by our rules cases in which arbitrary interference can be postulated.
28, 29. Consequent inappropriateness of many arguments.

CHAPTER XVIII.

STATISTICS AS APPLIED TO HUMAN ACTIONS.

§ 1. Are statistics fitly applicable to human actions?
2. Two conditions for such applicability.
3. (I.) The objects of the statistics must be left undisturbed.
4—6. This condition contravened by publication of results.
7, 8. Is this a practical objection?
9. (II.) The observer must not intrude himself amongst the statistics.
10—13. How such liability can arise.
14—16. Quotation from Buckle's History in illustration.
18, 19. Confusion between the speculative and practical standing points.
20—22. Consequent inducements to practical fatalism.
23. Conclusion.

THE LOGIC OF CHANCE.

CHAPTER I.

ON A CERTAIN KIND OF SERIES AS THE FOUNDATION OF PROBABILITY.

§ 1. IT is sometimes not easy to give a clear definition of a science at the outset, so as to set its scope and province before the reader in a few words. In the case of those sciences which are more immediately and directly concerned with objects, rather than with processes, this difficulty is not indeed so serious. If the reader is already familiar with the objects, a simple reference to them will give him a tolerably accurate idea of the direction and nature of his studies. Even if he be not familiar with them, they will still be often to some extent connected and associated in his mind by a name, and the mere utterance of the name may thus convey a fair amount of preliminary information. This is more or less the case with many of the natural sciences; we can often tell the reader beforehand exactly what he is going to study. But when a science is concerned, not so much with objects directly, as with processes and laws, or when it takes for the subject of its enquiry some comparatively obscure feature drawn from phenomena which have little or nothing else in common, the difficulty of giving preliminary information becomes greater. Recognized classes of objects have then to be disregarded and even broken up, and an entirely novel arrangement of the objects to be made. In such cases it is the study of the science that first gives the science its unity, for till it is studied the objects with which it is concerned were probably never thought of together. Here a definition cannot be given at the outset, and the process of obtaining it may become by comparison somewhat laborious.

The science of Probability, at least on the view taken of it in the following pages, is of this latter description. The reader who is at present unacquainted with it cannot be at once informed of its scope by a reference to objects with which he is already familiar. He will have to be taken in hand, as it were, and some little time and trouble will have to be expended in directing his attention to our subject-matter before he can be expected to know it. To do this will be our first task.

§ 2. In studying Nature, in any form, we are continually coming into possession of information which we sum up in general propositions. Now in very many cases these general propositions are neither more nor less certain and accurate than the details which they embrace and of which they are composed. We are assuming at present that the truth of these generalizations is not disputed; as a matter of fact they may rest on weak evidence, or they may be uncertain from their being widely extended by induction; what is meant is, that when we resolve them into their component parts we have precisely the same assurance of the truth of the details as we have of that of the whole. When I know, for instance, that all cows ruminate, I feel just as certain that any particular cow or cows ruminate as that the whole class does. I may be right or wrong in my original statement, and I may have obtained it by any conceivable mode in which truths can be obtained; but whatever the value of the general proposition may be, that of the particulars is neither greater nor less. The process of inferring the particular from the general is not accompanied by the slightest diminution of certainty. If one of these 'immediate inferences' is justified at all, it will be always right.

But it is by no means necessary that this characteristic should exist in all cases. There is a class of immediate inferences, almost unrecognized indeed in logic, but constantly drawn in practice, of which the characteristic is, that as they increase in particularity they diminish in certainty. Let me assume that I am told that *some* cows ruminate; I cannot infer logically from this that any particular cow does so, though I should feel some way removed from absolute disbelief, or even indifference to assent, upon the subject; but if I saw a herd of cows I should feel more sure that some of them were ruminant than I did of the single cow, and my assurance would increase with the numbers of the herd about which I had to form an opinion. Here then we have a class of things as to the individuals of which we feel quite in uncertainty, whilst as we embrace larger numbers in our assertions we attach greater weight to our inferences. It is with such classes of things and such inferences that the science of Probability is concerned.

§ 3. In the foregoing remarks, which are intended to be purely preliminary, we have not been able altogether to avoid some reference to a subjective element, viz. the degree of our certainty or belief about the things which we are supposed to contemplate. The reader may be aware that by some writers this element is regarded as the subject-matter of the science. Hence it will have to be discussed in a future chapter. As however I do not at all agree with the opinion of the writers just mentioned, at least as regards treating this element as one of primary importance, no further allusion will be made to it here, but we will pass on at once to a more minute investigation of that distinctive characteristic of certain classes of things which was introduced to notice in the last section.

In these classes of things, which are those with which Probability is concerned, the fundamental conception which the reader has to fix in his mind as clearly as possible, is, I take it, that of a series. But it is a series of a peculiar kind, one of which no better compendious description can be given than that which is contained in the statement that it combines individual irregularity with aggregate regularity. This is a statement which will probably need some explanation. Let us recur to an example of the kind already alluded to, selecting one which shall be in accordance with experience. Some children will not live to thirty. Now if this proposition is to be regarded as a purely indefinite or, as it would be termed in logic, particular proposition, no doubt the notion of a series does not obviously present itself in connection with it. It contains a statement about a certain unknown proportion of the whole, and that is all. But it is not with these purely indefinite propositions that we shall be concerned. Let us suppose the statement, on the contrary, to be of a numerical character, and to refer to a given proportion of the whole, and we shall then find it difficult to exclude the notion of a series. We shall find it, I think, impossible to do so as soon as we set before us the aim of obtaining accurate, or even moderately correct inferences. What, for instance, is the meaning of the statement that two children in three fail to attain the age of sixty-three? It certainly does not declare that in any given batch of, say thirty, we shall find just twenty that fail: whatever might be the strict meaning of the words, this is not the import of the statement. It rather contemplates our examination of a large number, of a long succession of instances, and states that in such a succession we shall find a numerical proportion, not indeed fixed and accurate at first, but which tends in the long run to become so. In every kind of example with which we shall be concerned we shall find this reference to a large number or succession of objects, or, as we shall term it, *series* of them.

A few additional examples may serve to make this plain.

Let us suppose that we toss up a penny a great many times; the results of the successive throws may be conceived to form a series. The separate throws of this series seem to occur in utter disorder; it is this disorder which causes our uncertainty about them. Sometimes head comes, sometimes tail comes; sometimes there is a repetition of the same face, sometimes not. So long as we confine our observation to a few throws at a time, the series seems to be simply chaotic. But when we consider the result of a long succession we find a marked distinction; a kind of order begins gradually to emerge, and at last assumes a distinct and striking aspect. We find in this case that the heads and tails occur in about equal numbers, that similar repetitions of different faces do so also, and so on. In a word, notwithstanding the individual disorder, an aggregate order begins to prevail. So again if we are examining the length of human life, the different lives which fall under our notice compose a series presenting the same features. The length of a single life is familiarly uncertain, but the average duration of a batch of lives is becoming in an almost equal degree familiarly certain. The larger the number we take out of any mixed crowd, the clearer become the symptoms of order, the more nearly will the average length of each selected class be the same. These few cases will serve as simple examples of a property of things which can be traced almost everywhere, to a greater or less extent, throughout the whole field of our ex-necessary to point out that we do not imply that the objects themselves which compose the series must occur successively in time; the series may be formed simply by their coming in succession under our notice, which as a matter of fact they may do in any order whatever. A register of mortality, for instance, may be made up of deaths which took place simultaneously or successively; or we might if we pleased arrange the deaths in an order quite distinct from either of these. This is entirely a matter of indifference; in all these cases the series, for any purposes which we need take into account, may be regarded as being of precisely the same description. The objects, be it remembered, are given to us in nature; the order under which we view them is our own private arrangement. This is mentioned here simply by way of caution, the meaning of this assertion will become more plain in the sequel.

I am aware that the word 'series' in the application with which it is used here is liable to some misconstruction, but I cannot find any better word, or indeed any as suitable in all respects. As remarked above, the events need not necessarily have occurred in a regular sequence of time, though they often will have done so. In many cases (for instance, the throws of a penny or a die) they really do occur in succession; in other cases (for instance, the heights of men, or the duration of their lives), whatever may have been the order of their actual occurrence, they are commonly brought under our notice in succession by being arranged in statistical tables. In all cases alike our processes of inference involve the necessity of examining one after another of the members which compose the group, or at least of being prepared to do this, if we are to be in a position to justify our inferences. The force of these considerations will come out in the course of the investigation in Chapter V.

Mr Leslie Ellis[1] has expressed what seems to me a substantially similar view in terms of genus and species, instead of speaking of a series. He says, "When individual cases are considered, we have no conviction that the ratios of frequency of occurrence depend on the circumstances common to all the trials. On the contrary, we recognize in the determining circumstances of their occurrence an extraneous element, an element, that is, extraneous to the idea of the genus and species. Contingency and limitation come in (so to speak) together; and both alike disappear when we consider the genus in its entirety, or (which is the same thing) in what may be called an ideal and practically impossible realization of all which it potentially contains. If this be granted, it seems to follow that the fundamental principle of the Theory of Probabilities may be regarded as included in the following statement,—The conception of a genus implies that of numerical relations among the species subordinated to it." As remarked above, this appears a substantially similar doctrine to that explained in this chapter, but I do not think that the terms genus and species are by any means so well fitted to bring out the conception of a tendency or limit as when we speak of a series, and I therefore much prefer the latter expression.

§ 6. The reader will now have in his mind the conception of a series of things or events, about the individuals of which we know but little, at least in certain respects, whilst we find a continually increasing uniformity as we take larger numbers under our notice. This is definite enough to point out tolerably clearly the kind of things with which we have to deal, but it is not sufficiently definite for purposes of accurate thought. We must therefore attempt a somewhat closer analysis.

---

[1] Transactions of the Cambridge Philosophical Society, Vol. IX. p. 605. Reprinted in the collected edition of his writings, p. 50.

There are certain phrases so commonly adopted, as to have become part of the technical vocabulary of the subject, such as an 'event' and the 'way in which it can happen.' Thus the act of throwing a penny would be called an event, and the fact of its giving head or tail would be called the way in which the event happened. If we were discussing tables of mortality, the former term would denote the mere fact of death, the latter the age at which it occurred, or the way in which it was brought about, or whatever else in it might be the particular circumstance under discussion. This phraseology is very convenient, and will often be made use of in this work, but without explanation it may lead to confusion. For in many cases the way in which the event happens is of such great relative importance, that according as it happens in one way or another the event would have a different name, in other words, it would not in the two cases be nominally the same event. The phrase therefore will have to be considerably stretched before it will conveniently cover all the cases to which we may have to apply it. If for instance we were contemplating a series of human beings, male and female, it would sound odd to call their humanity an event, and their sex the way in which the event happened.

If we recur however to any of the classes of objects already referred to, we may see our path towards obtaining a more accurate conception of what we want. It will easily be seen that in every one of them there is a mixture of similarity and dissimilarity; there is a series of events which have a certain number of features or attributes in common,—without this they would not be classed together. But there is also a distinction existing amongst them; a certain number of other attributes are to be found in some and are not to be found in others. In other words, the individuals which form the series are compound, each being made up of a collection of things or attributes; some of these things exist in all the members of the series, others are found in some only. So far there is nothing peculiar to the science of Probability; that in which the distinctive characteristic consists is this;—that the occasional attributes, as distinguished from the permanent, are found on an extended examination to exist *in a certain definite proportion of the whole number of cases.* We cannot tell in any given instance whether they will be found or not, but as we go on examining more cases we find a growing uniformity. We find that the proportion of instances in which they are found to instances in which they are wanting, is gradually subject to less and less variation, and approaches continually towards some apparently fixed value.

The above is the most comprehensive form of description; as a matter of fact the groups will in many cases take a far simpler form; they may appear, e. g. simply as a succession of things of the same kind, say human beings, with or without an occasional attribute, say that of being left-handed. We are using the word attribute, of course, in its widest sense, intending it to include every distinctive feature that can be observed in a thing, from essential qualities down to the merest accidents of time and place.

§ 7. On examining our series, therefore, we shall find that it may best be conceived, not necessarily as a succession of events happening in different ways, but as a succession of groups. These groups, on being analysed, are found in every case to be resolvable into collections of substances and attributes. That which gives its unity to the succession of groups is the fact of some of these substances or attributes being common to the whole succession; that which gives their distinction to the groups in the succession is the fact of some of them containing only a portion of these substances and attributes, the other portion or portions being occasionally absent. So understood, our phraseology may be made to embrace every class of things of which Probability can take account.

§ 8. It will be easily seen that the ordinary expression (viz. the 'event,' and the 'way in which it happens') may be included in the above. When the occasional attributes are unimportant the permanent ones are sufficient to fix and appropriate the name, the presence or absence of the others being simply denoted by some modification of the name or the addition of some predicate. We may therefore in all such cases speak of the collection of attributes as 'the event,'—the same event essentially, that is—only saying that *it* (so as to preserve its nominal identity) happens in different ways in the different cases. When the occasional attributes however are important, or compose the majority, this way of speaking becomes less appropriate; language is somewhat strained by our implying that two extremely different assemblages are in reality the same event, with a difference only in its mode of happening. The phrase is however a very convenient one, and with this caution against its being misunderstood, it will frequently be made use of here.

§ 9. A series of the above-mentioned kind is, I apprehend, the ultimate basis upon which all the rules of Probability must be based. It is essential to a clear comprehension of the subject to have carried our analysis up to this point, but any attempt at further analysis into the intimate nature of the events composing the series, is not required. It is altogether unnecessary, for instance, to form any opinion upon the questions discussed in metaphysics as to the independent existence of substances. We have discovered, on examination, a series composed of groups of substances and attributes, or of attributes alone. At such a series we stop, and thence investigate our rules of inference; into what these substances or attributes would themselves be ultimately analysed, if taken in hand by the psychologist or metaphysician, it is no business of ours to enquire here.

§ 10. The stage then which we have now reached is that of having discovered a quantity of things (they prove to be groups on analysis) which are capable of being classified together, and are best regarded as a series. The distinctive peculiarity of this series is our finding in it an order, gradually emerging out of disorder, and showing in time a marked and unmistakeable uniformity. The impression which may possibly be derived from the description of such a series, and which the reader will probably already entertain if he have studied Probability before, is that the gradual evolution of this order is indefinite, and its approach therefore to perfection unlimited. And many of the examples commonly selected certainly tend to confirm such an impression. But in reference to the theory of the subject it is, I am convinced, an error, and one liable to lead to much confusion.

The lines which have been prefixed as a motto to this work, "So careful of the type she seems, so careless of the single life," are soon after corrected by the assertion that the type itself, if we regard it for a long time, changes, and then vanishes and is succeeded by others. So in Probability; that uniformity which is found in the long run, and which presents so great a contrast to the individual disorder, though durable is not everlasting. Keep on watching it long enough, and it will be found almost invariably to fluctuate, and in time may prove as utterly irreducible to rule, and therefore as incapable of prediction, as the individual cases themselves. The full bearing of this fact upon the theory of the subject, and upon certain common modes of calculation connected with it, will appear more fully in some of the following chapters; at present we will confine ourselves to very briefly establishing and illustrating it.

Let us take, for example, the average duration of life. This, provided our data are sufficiently extensive, is known to be tolerably regular and uniform. This has been already indicated in the preceding sections, and is a truth indeed of which the popular mind has a tolerably clear grasp at the present day. But a very little consideration will show that there may be a superior as well as an inferior limit to the extent within which this uniformity can be observed; in other words whilst we may fall into error by taking too few instances we may also fail in our aim, though in a very different way and from different reasons, by taking too many. At the present time the average duration of life in England may be, say thirty; but a century ago it was decidedly less; several centuries ago it was presumably very much less; whilst if we possessed statistics referring to a still earlier population of the country we should probably find that there has been since that time a still more marked improvement. What may be the future tendency no man can say for certain. It may be, and we hope will be the case, that owing to sanitary and other improvements, the duration of life will go on increasing steadily; it is at least conceivable, though doubtless incredible, that it should do so without limit. On the other hand, this duration might gradually tend towards some fixed length. Or, again, it is perfectly possible that future generations might prefer a short and a merry life, and therefore reduce their average. The duration of life cannot but depend to some extent upon the general tastes, habits and employments of the people, that is upon the ideal which they consciously or unconsciously set before them, and he would be a rash man who should undertake to predict what this ideal will be some centuries hence. All that it is here necessary however to indicate is, that this uniformity (as we have hitherto called it, in order to mark its relative character) has varied, and, under the influence of future eddies in opinion and practice, may vary still; and this to any extent, and with any degree of irregularity. To borrow a term from Astronomy, we find our uniformity subject to what might be called an irregular *secular* variation.

§ 11. The above is a fair typical instance. If we had taken a less simple feature than the length of life, or one less closely connected with what may be called by comparison the great permanent uniformities of nature, we should have found the peculiarity under notice exhibited in a far more striking degree. The deaths from small-pox, for example, or the instances of duelling or accusations of witchcraft, if examined during a few successive years, might have shown a very tolerable degree of uniformity. But these uniformities have risen probably from zero; after various and very great fluctuations seem tending towards zero again, at least in this country; and may, for anything we know, undergo still greater fluctuations in future. Now these examples must be regarded as being only extreme ones, and not such very extreme ones, of what is the almost universal rule in nature. I shall endeavour to show that even the few apparent exceptions, such as the proportions between male and female births, &c., may not be, and probably in reality are not, strictly speaking, exceptions. A type that is, in the fullest sense of the words, persistent and invariable is scarcely to be found in nature. The full import of this conclusion will be seen in future chapters. Attention is only directed here to the important inference that, although statistics are notoriously of no value unless they are in sufficient numbers, yet it does not follow but that in certain cases we may have too many of them. If they are made too extensive, they may again fall short, at least for any particular time or place, of their greatest attainable accuracy.

§ 12. These natural uniformities then are found at length to be subject to fluctuation. Now contrast with them any of the uniformities afforded by games of chance; these latter seem to show no trace of secular fluctuation, however long we may continue our examination of them. Criticisms will be offered, in the course of the following chapters, upon some of the common attempts to prove *à priori* that there must be this fixity in the uniformity, but of its existence there can scarcely be much doubt. Pence give heads and tails equally often now, as they did when they were first tossed, and as we believe they will continue to do, so long as the present order of things continues. The fixity of these uniformities may not be as absolute as is commonly supposed, but no amount of experience which we need take into account is likely in any appreciable degree to interfere with them. Hence the contrast, that, whereas natural uniformities at length fluctuate, those afforded by games of chance seem fixed for ever.

§ 13. Here then are series apparently of two different kinds. They are alike in their initial irregularity, alike in their subsequent regularity; it is in what we may term their ultimate form that they begin to diverge from one another. The one tends without any irregular variation towards a fixed numerical proportion in its uniformity; in the other the uniformity is found at last to fluctuate, and to fluctuate, it may be, in a manner utterly irreducible to rule.

As this chapter is intended to be little more than explanatory and illustrative of the foundations of the science, the remark may be made here (for which subsequent justification will be offered) that it is in the case of series of the former kind only that we are able to make anything which can be interpreted into strict scientific inferences. We shall be able however in a general way to see the kind and extent of error that would be committed if, in any example, we were to substitute an imaginary series of the former kind for any actual series of the latter kind which experience may present to us. The two series are of course to be as alike as possible in all respects, except that the variable uniformity has been replaced by a fixed one. The difference then between them would not appear in the initial stage, for in that stage the distinctive characteristics of the series of Probability are not apparent; all is there irregularity, and it would be as impossible to show that they were alike as that they were different; we can only say generally that each shows the same kind of irregularity. Nor would it appear in the subsequent stage, for the real variability of the uniformity has not for some time scope to make itself perceived. It would only be in what we have called the ultimate stage, when we suppose the series to extend for a very long time, that the difference would begin to make itself felt[1]. The proportion of persons, for example, who die each year at the age of six months is, when the numbers examined are on a small scale, utterly irregular; it becomes however regular when the numbers examined are on a larger scale; but if we continued our observation for a very great length of time, or over a very great extent of country, we should find this regularity itself changing in an irregular way. The substitution just mentioned is really equivalent to saying, Let us assume that the regularity is fixed and permanent. It is making a hypothesis which may not be altogether consistent with fact, but which is forced upon us for the purpose of securing precision of statement and definition.

---

[1] We might express it thus:—a few instances are not sufficient to display a law at all; a considerable number will display it; but it takes a very great number to establish that a *change* is taking place in the law.

§ 14. The full meaning and bearing of such a substitution will only become apparent in some of the subsequent chapters, but it may be pointed out at once that it is in this way only that we can introduce strictly the notion of a 'limit' into our account of the matter, at any rate in reference to many of the applications of the subject to purely statistical enquiries. We say that a certain proportion begins to prevail among the events in the long run; but then on looking closer at the facts we find that we have to express ourselves hypothetically, and to say that if present circumstances remain as they are, the long run will show its characteristics without disturbance. When, as is often the case, we know nothing accurately of the circumstances by which the succession of events is brought about, but have strong reasons to suspect that these circumstances are likely to undergo some change, there is really nothing else to be done. We can only introduce the conception of a limit, towards which the numbers are tending, by assuming that these circumstances do not change; in other words, by substituting a series with a fixed uniformity for the actual one with the varying uniformity[1].

---

[1] The mathematician may illustrate the nature of this substitution by the analogies of the 'circle of curvature' in geometry, and the 'instantaneous ellipse' in astronomy. In the cases in which these conceptions are made use of we have a phenomenon which is continuously varying and also

§ 15. If the reader will study the following example, one well known to mathematicians under the name of the Petersburg[1] problem, he will find that it serves to illustrate several of the considerations mentioned in this chapter. It serves especially to bring out the facts that the series with which we are concerned must be regarded as indefinitely extensive in point of number or duration; and that when so regarded certain series, but certain series only (the one in question being a case in point) take advantage of the indefinite range to keep on producing individuals in it whose deviation from the average has no finite limit whatever. When rightly viewed it is a very simple problem, but it has given rise, at one time or another, to a good deal of confusion and perplexity.

The case is this:—a penny is tossed up; if it gives head I receive two shillings; if heads twice running four shillings; if heads three times running eight shillings, and so on; the amount to be received doubling every time that a fresh head succeeds. In a word, I am to go on as long as it continues to give heads, to regard this succession as a 'turn' or set, and then take another turn, and so on; and for each turn I am to receive a payment. However many times head may be given in succession, the number of shillings I may claim ? n. is found by multiplying two by itself that number of times. Here then is a series formed by a succession of throws. We will assume,—what many persons will consider to admit of demonstration, and what certainly experience confirms within considerable limits,—that the rarity of these 'runs' of the

---

changing its rate of variation. We take it at some given moment, suppose its rate at that moment to be fixed, and then complete its career on that supposition.

[1] So called from its first appearing in the *Commentarii* of the Petersburg Academy; a variety of notices upon it will be found in Mr Todhunter's History of the theory of Probability.

same face is in direct proportion to the amount I receive for them when they do occur. In other words, if we regard only the occasions on which I receive payments, we shall find that every other time I get two shillings, once in four times I get four shillings, once in eight times eight shillings, and so on without any end. The question is then asked, what ought I to pay for this privilege? At the risk of a slight anticipation of the results of a subsequent chapter, we may assume that this is equivalent to asking, what amount paid each time would on the average leave me neither winner nor loser? In other words, what is the average amount I should receive on the above terms? Theory pronounces that ought to give an *infinite* sum, that is, that no finite sum however great, would be an adequate equivalent. And thi is really quite intelligible. There is a series of indefinit length before me, and the longer I continue to work it th richer are my returns, and this without any limit whatever It is true that the very rich hauls are extremely rare, bu still they do come, and when they come they make it up by their greater richness. On every occasion on which people have devoted themselves to the pursuit in question, they made acquaintance, of course, with but a limited portion of this series; but the series on which we base our calculation is unlimited; and the inferences usually drawn as to the sum which ought in the long run to be paid for the privilege in question are in perfect accordance with this supposition.

The common form of objection is given in the reply, that so far from paying an infinite sum, no sensible man would give anything approaching to £50 for such a chance. Probably not, because no man would see enough of the series to make it worth his while. What most persons form thei practical opinion upon, is such small portions of the serie as they have actually seen or can reasonably expect. Now in any such portion, say of 100 turns, the longest succession of heads would not amount on the average to more than seven or eight. This is observed, but it is forgotten that the formula which produced these, would, if it had greater scope, keep on producing better and better ones without any limit. Hence it arises that some persons are perplexed, because the conduct they would adopt, in reference to the curtailed portion of the series which they are practically likely to meet with, does not find its justification in inferences which are necessarily based upon the series in the completeness of its infinitude. The problem will have to be referred to again in a future chapter.

CHAPTER II.

FURTHER DISCUSSION UPON THE NATURE OF THE SERIES MENTIONED IN THE LAST CHAPTER.

§ 1. In the course of the last chapter the nature of a particular kind of series, that namely, which must be considered to constitute the basis of the science of Probability, has received a sufficiently general explanation for the preliminary purpose of introduction. One might indeed say more than this, for the characteristics which were there pointed out are really sufficient in themselves to give a fair general idea of the nature of Probability, and of the sort of problems with which it deals. But in the concluding paragraphs an indication was given that the series of this kind, as they actually occur in nature or as the results of more or less artificial production, are seldom or never found to occur in such a simple form as might possibly be expected from what had previously been said; but that they are almost always seen to be associated together in groups after a somewhat complicated fashion. A fuller discussion of this topic must now be undertaken.

We will take for examination an instance of a kind with which the investigations of Quetelet will have served to familiarize some readers. Suppose that we measure the heights of a great many adult men in any town or country. These heights will of course lie between certain extremes in each direction, and if we continue to accumulate our measures it will be found that they tend to lie continuously between these extremes; that is to say, that under those circumstances no intermediate height will be found to be permanently unrepresented in such a collection of measurements. Now suppose these heights to be marshalled in the order of their magnitude. What we always find is something of the following kind;—about the middle point between the extremes, a large number of the results will be found crowded together: a little on each side of this point there will still be an excess, but not to so great an extent; and so on, in some diminishing scale of proportion, until as we get towards the extreme results the numbers thin off and become relatively exceedingly small.

The point to which attention is here directed is not the mere fact that the numbers thus tend to diminish from the middle outwards, but, as will be more fully explained directly, the *law* according to which this progressive diminution takes place. The word 'law' is here used in its mathematical sense, to express the formula connecting together the two elements in question, namely, the height itself, and the relative number that are found of that height. We shall have to enquire whether one of these elements is a function of the other, and, if so, what function.

§ 2. After what was said in the last chapter, it need hardly be insisted upon that the interest and significance of such investigations as these are almost entirely dependent upon the statistics being very extensive. In one or other of Quetelet's works on Social Physics[1] will be found a selection of measurements of almost every element which the physical frame of man can furnish:—his height, his weight, the muscular power of various limbs, the dimensions of almost ever part and organ, and so on. Some of the most extensive these express the heights of 25,000 Federal soldiers from th Army of the Potomac, and the circumferences of the chest of 5738 Scotch militia men taken some years ago. Thos who wish to consult a large repertory of such statistics can not be referred to any better sources than to these and othe works by the same author.

---

[1] *Essai de Physique Sociale*, 1869. *Anthropométrie*, 1870.

Interesting and valuable, however, as are Quetelet's statistical investigations (and much of the importance now deservedly attached to such enquiries is, doubtless, owing more to his efforts than to those of any other person), I cannot but feel convinced that there is much in what he ha written upon the subject which is erroneous and confusing as regards the foundations of the science of Probability, and th philosophical questions which it involves. These errors ar not by any means confined to him, but for various reason they will be better discussed in the form of a criticism of hi explicit or implicit expression of them, than in any more independent way.

§ 3. In the first place then, he always, or almost always assumes that there can be but one and the same law of arrangement for the results of our observations, measurements and so on, in these statistical enquiries[1]. That is, he assumes that whenever we get a group of such magnitudes clustering about a mean, and growing less frequent as they depart from that mean, we shall find that this diminution of frequency takes place according to one invariable law, whatever may be the nature of these magnitudes, and whatever the process by which they may have been obtained.

---

[1] About the only exception that he makes, so far as I am aware, is that of our weights; which are certainly not distributed symmetrically according to his figures; that is to say, out of any large total, the numbers of those who are equally in excess and in defect of the mean, by any given amount, are by no means equal.

That such a uniformity as this should prevail amongst many and various classes of phenomena would probably seem surprising in any case. But the full significance of such a fact as this (if indeed it be a fact) only becomes apparent when attention is directed to the profound distinctions in the nature and origin of the phenomena which are thus supposed to be harmonized by being brought under one comprehensive principle. This will be better appreciated if we take a brief glance at some of the principal classes into which the things with which Probability is chiefly concerned may be divided. These are of a three-fold kind.

§ 4. In the first place there are the various combinations, and runs of luck, afforded by games of chance. Suppose a handful, consisting of ten coins, were tossed up a great many times in succession, and the results were tabulated. What we should obtain would be something of the following kind. In a certain proportion of cases, and these the most numerous of all, we should find that we got five heads and five tails; in a somewhat less proportion of cases we should have, as equally frequent results, four heads six tails, and four tails six heads; and so on in a continually diminishing proportion until at length we came down, in a very small relative number of cases, to nine heads one tail, and nine tails one head; whilst the least frequent results possible would be those which gave all heads or all tails[1]. Here the statistical elements under consideration are, as regards their origin at any rate, optional or brought about by human choice. They would, therefore, be commo described as being mainly artificial, but their results u mately altogether a matter of chance.

---

[1] As every mathematician knows, the relative numbers of each of these possible throws are given by the successive terms of the expansion of $(1+1)^{10}$, viz. 1, 10, 45, 120, 210, 252, 210, 120, 45, 10, 1.

Again, in the second place, we might take the accur measurements of a great many natural objects belongi to the same genus or class; such as the cases, already ferred to, of the heights, or other characteristics of the habitants of any district. Here human volition or int vention of any kind seem to have little or nothing to with the matter. It is optional with us to *collect* the m sures, but the things measured are quite outside our c trol. They would therefore be commonly described being altogether the production of nature, and it wo not be supposed that in strictness chance had anythi whatever to do with the matter.

In the third place, the result at which we are aimi may be some fixed magnitude, one and the same in ea of our successive attempts, so that if our measureme were rigidly accurate we should merely obtain the sa result repeated over and over again. But since all c methods of attaining our aims are practically subject innumerable imperfections, the results actually obtain will depart more or less, in almost every case, from t real and fixed value which we are trying to secure. Th will be sometimes more wide of the mark, sometimes l so, the worse attempts being of course the less freque If a man aims at a target he will seldom or never hit precisely in the centre, but his good shots will be mo

---

[1] That is they will be more densely aggregated. If a space *the size of the bull's-eye* be examined in each successive circle, the number of shot marks which it contains will be successively less. The *actual* num of shots which strike the bull's- will not be the greatest, since it co so much less surface than any of other circles.

numerous than his bad ones. Here again, then, we have a series of magnitudes clustering about a mean, but produced in a very different way from those of the last two cases. In this instance the elements would be commonly regarded as only partially the results of human volition, and chance therefore as being only a co-agent in the effects produced.

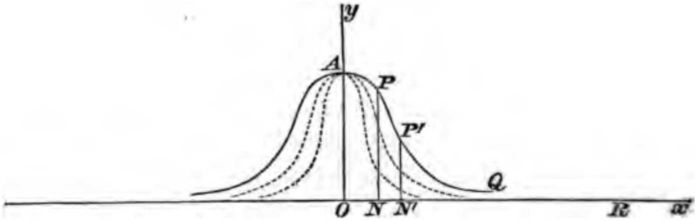
Other classes of things, besides those alluded to above, might readily be given. These however are the ones about which the most extensive statistics are obtainable, or to which the most practical importance and interest are attached. The profound distinctions which separate their origin and character are obvious. If they all really did display precisely the same law of variation it would be a most remarkable fact, pointing doubtless to some deep-seated identity underlying the various ways, apparently so widely distinct, in which they had been brought about. The questions now to be discussed are; Is it the case, with any considerable degree of rigour, that only one law of distribution does really prevail? and, in so far as this is so, how does it come to pass?

§ 5. In support of an affirmative answer to the former of these two questions, several different kinds of proof are, or might be, offered.

(I.) For one means we may make a direct appeal to experience, by collecting sets of statistics and observing what is their law of distribution. As remarked above, this has been done in a great variety of cases, and in some instances to a very considerable extent, by Quetelet and others. His researches have made it abundantly convincing that many classes of things and processes, differing widely in their nature and origin, do nevertheless appear to conform with a considerable degree of accuracy to one and the same[1] law. At least this is made plain for the more

---

[1] Commonly called the exponential law; its equation being of the form $y=Ae^{-hx^2}$. The curve corresponding to it cuts the axis of y at right angles (expressing the fact that near the mean there are a large number of values approximately equal; after a time it begins to slope away rapidly towards the axis of x (expressing the fact that the results soon begin to grow less common as we recede from the mean); and the axis of x is an asymptote in both directions (expressing the fact that no magnitude, however remote from the mean, is strictly impossible; that is, every deviation, however excessive, will have to be encountered at length within the range of a sufficiently long experience). The curve is obviously symmetrical, expressing the fact that equal deviations from the mean, in excess and in defect, tend to occur equally often in the long run.



A rough graphic representation of the curve is given above. For the benefit of those unfamiliar with mathematics one or two brief remarks may be here appended concerning some of its properties. (1) It must not be supposed that all specimens of the curve are similar to one another. The dotted lines are equally specimens of it. In fact, by varying the essentially arbitrary units in which x and y are respectively estimated, we may make the vertex of the curve as obtuse or as acute as we please. This consideration is of importance; for it reminds us that, by varying one of these arbitrary units, we could get an 'exponential curve' which should tolerably closely resemble any other given curve of error, provided that this latter recognized and was founded upon the assumption that extreme divergences were excessively rare. Hence it would be difficult, by mere observation, to prove that the law of error in any given case was not exponential; unless the statistics were very extensive, or the actual results departed considerably from the exponential form. (2) It is quite impossible by any graphic representation to give an adequate idea of the excessive rapidity with which the curve after a time approaches the axis of x. At the point R, on our scale, the curve would approach within the fifteen-thousandth part of an inch from the axis of x, a distance which only a very central values, for those that is which are situated most nearly about the mean. With regard to the extreme values there is, on the other hand, some difficulty. For instance in the arrangements of the heights of a number of men, these extremes are rather a stumbling-block; indeed it has been proposed to reject them from both ends of the scale on the plea that they are monstrosities, the fact being that their relative numbers do not seem to be by any means those which theory would assign[1]. Such a plan of rejection is however quite unauthorized, for these dwarfs and giants are born into the world like their more normally sized brethren, and have precisely as much right as any others to be included in the formulæ we draw up.

Besides the instance of the heights of men, other classes of observations of a somewhat similar character have been already referred to as collected and arranged by Quetelet. From the nature of the case, however, there are not many appropriate ones at hand; for when our object is, not to illustrate a law which can be otherwise proved, but to obtain actual direct proof of it, the collection of observations and measurements ought to be made upon such an enormous scale as to deter any but the most persevering computers from undergoing the requisite labour. Some of the remarks made in the course of the note on the opposite page will serve to illustrate the difficulties which would lie in the way of such a mode of proof. I am hardly aware, indeed, of any instances which seem with thorough satisfaction to fulfil all the necessary conditions.

---

good microscope could detect. Whereas in the hyperbola e.g. the rate of approach of the curve to its asymptote is continually decreasing, it is here just the reverse; this rate is continually increasing. Hence the two, viz. the curve and the axis of x, appear to the eye, after a very short time, to merge into one another.

[1] As by Quetelet: noted, amongst others, by Herschel, *Essays*, page 409.

§ 6. Allowing the various statistics such credit as th
deserve, for their extent, appropriateness, accuracy and
on, the general conclusion which will on the whole be dra
by almost every one who takes the trouble to consult the
is that they do, as a rule, conform approximately to o
type or law, at any rate for all except the extreme valu
So much as this must be fully admitted. But that they
not, indeed we may say that they cannot, always do so
the case of the extreme values, will become obvious
a little consideration. In some of the classes of things
which the law is supposed to apply, for example the su
cessions of heads and tails in the throws of a penny, there
no limit to the magnitude of the fluctuations which may a
will occur. Postulate as long a succession of heads or of ta
as we please, and if we could only live and wait long enou
for it we should succeed in getting it at length. In ot
cases, including many of the applications of Probabil
to natural phenomena, there can hardly fail to be su
limits. Deviations exceeding a certain range may not
merely improbable, that is of very rare occurrence, but th
may often from the nature of the case be actually imp
sible. And even when they are not actually impossible
may frequently appear on examination that they are o
rendered possible by the occasional introduction of agenc
which are not supposed to be available in the producti
of the more ordinary or intermediate values. When,
instance, we are making observations with any kind
instrument, the nature of its construction may put
absolute limit upon the possible amount of error. And ev
if there be not an absolute limit under all kinds of us
it may nevertheless be the case that there is one un
fair and proper usage; it being the case that only wh
the instrument is designedly or carelessly tampered with w

any new causes of divergence be introduced which were not confined within the old limits.

Suppose, for instance, that a man is firing at a mark. His worst shots must be supposed to be brought about by a combination of such causes as were acting, or prepared to act, in every other case. The extreme instance of what we may thus term 'fair usage' being when a number of distinct causes have happened to conspire together so as to tend in the same direction, instead of, as in the other cases, more or less neutralizing one another's work. But the aggregate effect of such causes may well be supposed to be limited. The man will not discharge his shot nearly at right angles to the true line of fire unless some entirely new cause comes in, as by some unusual circumstance having distracted his attention, or by his having had some spasmodic seizure. But influences of this kind were not supposed to have been available before; and even if they were we are taking a bold step in assuming that these occasional great disturbances are subject to the same kind of laws as are the aggregates of innumerable little ones.

We cannot indeed lay much stress upon an example of this last kind, as compared with those in which we can see for certain that there is a fixed limit to the range of error. It is therefore offered rather for illustration than for proof. The enormous, in fact inconceivable magnitude of the numbers expressive of the chance of very rare combinations, such as those in question, has such a bewildering effect upon the mind that one may be sometimes apt to confound the impossible with the higher degrees of the merely mathematically improbable.

§ 7 (II.). The last remarks will suggest another kind of proof which might be offered to establish the invariable nature of the law of error. It is of a direct deductive kind, not appealing immediately to statistics, but involving an enqu into the actual or assumed nature of the causes by which t events are brought about. Imagine that the event und consideration is brought to pass, in the first place, by som fixed cause, or group of fixed causes. If this comprised the influencing circumstances the event would invariab happen in precisely the same way: there would be no erro or deflections whatever to be taken account of. But n suppose that there were also an enormous number of ve small causes which tended to produce deflections; that the causes acted in entire independence of one another; and th each of the lot told as often, in the long run, in one directi as in the opposite. It is easy[1] to see, in a general way, wh would follow from these assumptions. In a very few ca nearly all the causes would tell in the same direction; other words, in a very few cases the deflection would extreme. In a greater number of cases, however, it wo only be the most part of them that would tell in one dir tion, whilst a few did what they could to counteract the re the result being a comparatively larger number of somewh smaller deflections. So on, in increasing numbers, till approach the middle point. Here we shall have a very la number of very small deflections: the cases in which opposed influences just succeed in balancing one anoth and so no error whatever is produced, being though actua infrequent, relatively the most frequent of all.

Now if all deflections from a mean were brought abou the way just indicated (an indication which must suffice the present) we should always have one and the same law of arrangement of frequency for these deflections or errors, viz. the exponential[1] law mentioned in § 5.

---

[1] The above reasoning will probably be accepted as valid at this stage of enquiry. But in strictness, assumptions are made here, which however justifiable they may be in themselves, involve somewhat of an e cipation. They demand, and future chapter will receive, cl scrutiny and criticism.

§ 8. It may be readily admitted from what we know about the production of events that something resembling these assumptions, and therefore something resembling the consequences which follow from them, is really secured in a very great number of cases. But although this may prevail approximately, it is in the highest degree improbable that it could ever be secured, even artificially, with anything approaching to rigid accuracy. For one thing, the causes of deflection will seldom or never be really independent of one another. Some of them will generally be of a kind such that the supposition that several are swaying in one direction, may affect the capacity of each to produce that full effect which it would have been capable of if it had been left to do its work alone. In the common example, for instance, of firing at a mark, so long as we consider the case of the tolerably good shots the effect of the wind (one of the causes of error) will be approximately the same whatever may be th precise direction of the bullet. But when a shot is conside ably wide of the mark the wind can no longer be regarded a acting at right angles to the line of flight, and its effect i consequence will not be precisely the same as before. I other words, the causes here are not strictly independent, a they were assumed to be; and consequently the results to b attributed to each are not absolutely uninfluenced by thos of the others. Doubtless the effect is trifling here, but apprehend that if we were carefully to scrutinize the mode in which the several elements of the total cause conspir together, we should find that the assumption of absolut independence was hazardous, not to say unwarrantable, in very great number of cases. These brief remarks upon th process by which the deflections are brought about mus suffice for the present purpose, as the subject will receive fuller investigation in the course of the next chapter.

---

[1] A definite numerical example of this kind of concentration of frequency about the mean was given in the note to § 4. It was of a binomial form, consisting of the successive terms of the expansion of $(1+1)^m$. Now it may be shown (Quetelet, *Letters*, p. 263; Liagre, *Calcul des Probabilités*, § 34) that the expansion of such a binomial, as m becomes indefinitely great, approaches as its limit the exponential form; that is, if we take a number of equidistant ordinates proportional respectively to 1, m, $\frac{m(m-1)}{1\cdot 2}$ &c., and connect their vertices, the figure we obtain approximately represents the curve $y = Ae^{-hx^2}$, and tends to become identical with it, as m is increased without limit. In other words, if we suppose the errors to be produced by a limited number of finite causes, we have an approximation to the exponential Law of Error, which merges into identity as the causes are increased in number and diminished in magnitude without limit. Mr Jevons has given (*Principles of Science*, Vol. I. p. 442) a diagram drawn to scale, to show how rapid this approximation is.

According, therefore, to the best consideration whic can at the present stage be afforded to this subject, we ma draw a similar conclusion from this deductive line of argument as from the direct appeal to statistics. The sam general result seems to be established; namely, that approximately, with sufficient accuracy for all practical purposes, w may say that an examination of the causes by which th deflections are generally brought about shows that they ar mostly of such a character as would result in giving us th commonly accepted 'Law of Error,' as it is termed[1]. Th two lines of enquiry, therefore, within the limits assigned, afford one another a decided mutual confirmation.

---

[1] 'Law of Error' is the usual technical term for what has been elsewhere spoken of above as a Law of Divergence from a mean. It is in strictness only appropriate in the case of one, namely the third, of the three classes of phenomena mentioned in § 4, but by a convenie generalization it is equally appli to the other two; so that we ter the amount of the divergence fro the mean an 'error' in every cas however it may have been broug about.

§ 9 (III.). There still remains a third, indirect and mathematical line of proof, which might be offered to establish the conclusion that the Law of Error is always one and the same. It may be maintained that the recognized and universal employment of one and the same method, that known to mathematicians and astronomers as the Method of Least Squares, in all manner of different cases with very satisfactory results, is only compatible with the supposition that the errors to which that method is applied must be grouped according to one invariable law. If all 'laws of error' were not of one and the same type, that is, if the relative frequency of large and small divergences (such as we have been speaking of) were not arranged according to one pattern, how could one method or rule equally suit them all?

In order to preserve a continuity of treatment, some notice must be taken of this enquiry here, though, as in the case of the last argument, any thorough discussion of the subject is impossible at the present stage. For one thing, it would involve too much employment of mathematics, or at any rate of mathematical conceptions, to be suitable for the general plan of this treatise: I have accordingly devoted a special chapter to the consideration of it.

The main reason, however, against discussing this argument here,—namely that to do so would involve the anticipation of a totally different side of the science of Probability from that hitherto treated of,—is one which I think really contains by implication an answer as against its validity. This must be especially insisted upon, as the neglect of it involves much confusion and some error. During these earlier chapters we have been entirely occupied with laying what may be called the physical foundations of Probability. We have done nothing else than establish, in one way or another, the existence of certain groups or arrangements of things which are found to present themselves in nature; we have endeavoured to explain how they come to pass, and we have illustrated their principal characteristics. But these are merely the foundations of Inference, we have not yet said a word upon the logical processes which are to be erected upon these foundations. We have not therefore entered yet upon the *logic* of chance.

§ 10. Now the way in which the Method of Least Squares is sometimes spoken of tends to conceal the magnitude of this distinction. Writers have used it as synonymous with the Law of Error, whereas the fact is that the two are not only totally distinct things but that they have scarcely even any necessary connection with one another. The Law of Error is the statement of a physical fact; it simply assigns, with more or less of accuracy, the relative frequency with which errors or deviations of any kind are found in practice to present themselves. It belongs therefore to what may be termed the physical foundations of the science. The Method of Least Squares, on the other hand, is not a law at all in the scientific sense of the term. It is simply a rule or direction informing us how we may best proceed to treat any group of these errors which may be set before us, so as to extract the true result at which they have been aiming. Clearly therefore it belongs to the inferential or logical part of the subject.

It cannot indeed be denied that the methods we employ must have some connection with the arrangement of the facts to which they are applied, but the two things are none the less distinct in their nature, and in this case the connection does not seem at all a necessary one, but at most one of propriety and convenience. The Method of Least Squares is usually applied, no doubt, to the most familiar and common form of the Law of Error, namely the exponential form with which we have been recently occupied. But other forms of laws of error may exist, and, if they did, the method in question might equally well be applied to them. I am not asserting that it would necessarily be the best method in every case, but it would be a possible one; indeed we may go further and say, as will be shown in a future chapter, that it would be a good method in every case. But its particular merits or demerits do not interfere with its possible employment in every case in which we may choose to resort to it. It will be seen therefore, even from the few remarks that can be made upon the subject here, that the fact that one and the same method is very commonly employed with satisfactory results affords little or no proof that the errors to which it is applied must be arranged according to one fixed law.

§ 11. So much then for the attempt to prove the prevalence, in all cases, of this particular law of divergence. The next point in Quetelet's treatment of the subject which deserves attention as erroneous or confusing, is the doctrine maintained by him and others as to the existence of what he terms a *type* in the groups of things in question. This is a not unnatural consequence from some of the data and conclusions of the last few paragraphs. Refer back to two of the three classes of things already mentioned in § 4. If it really were the case that in arranging in order a series of incorrect observations or attempts of our own, and a collection of natural objects belonging to some one and the same species or class, we found that the law of their divergence was in each case identical in the long run, we should be naturally disposed to apply the same expression 'Law of Error' to both instances alike, though in strictness it could only be appropriate to the former. When we perform an operation ourselves with a clear consciousness of what we are aiming at, we may quite correctly speak of every deviation from this as being an error; but when Nature presents us with a group of objects of any kind, it is using a rather bold metaphor to speak in this case also of a law of error, as if she had been aiming at something all the time, and had like the rest of us missed her mark more or less in almost every instance[1].

Suppose we make a long succession of attempts to measure accurately the precise height of a man, we should from one cause or another seldom or never succeed in doing so with absolute accuracy. But we have no right to assume that these imperfect measurements of ours would be found so to deviate according to one particular law of error as to present the precise counterpart of a series of actual heights of *different* men, supposing that these latter were assigned with absolute precision. What might be the actual law of error in a series of direct measurements of any given magnitude could hardly be asserted beforehand, and probably the attempt to determine it by experience has not been made sufficiently often to enable us to ascertain it; but upon general grounds it seems highly unlikely that it would follow the so-called exponential law. Be this however as it may, it is rather a licence of language to talk as if nature had been at work in the same way as one of us; aiming (ineffectually for the most part) at a given result, that is at producing a man endowed with a certain stature, proportions, and so on, who might therefore be regarded as the typical man.

§ 12. Stated as above, namely that there is a fixed invariable human type to which all individual specimens of humanity may be regarded as having been meant to attain, but from which they have deviated in one direction or another, according to a law of deviation capable of *à priori* determination, the doctrine is little else than absurd. But if we look somewhat closer at the facts of the case, and the probable explanation of these facts, we may see our way to an important truth. The facts, on the authority of Quetelet's statistics (the interest and value of which must be frankly admitted), are very briefly as follows: if we take any element of our physical frame which admits of accurate measurement, say the height, and determine this measure in a great number of different individuals belonging to any tolerably homogeneous class, we shall find that these heights do admit of an orderly arrangement about a mean, after the fashion which has been already repeatedly mentioned. What is meant by a homogeneous class? is a pertinent and significant enquiry, but applying this condition to any simple cases its meaning is readily stated. It implies that the mean in question will be different according to the nationality of the persons under measurement. According to Quetelet[1], in the case of Englishmen the mean is about 5 ft. 9 in.; for Belgians about 5 ft. 7 in.; for the French about 5 ft. 4 in. It need hardly be added that these measures are those of adult males.

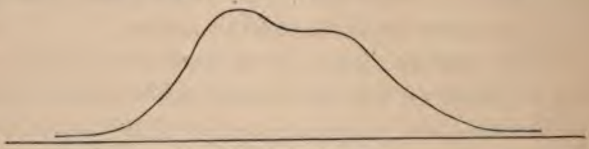
---

[1] This however seems to be the purport, either by direct assertion or by implication, of two elaborate works by Quetelet, viz. his *Physique Sociale*, and his *Anthropométrie*.

§ 13. It may fairly be asked here what would have been the consequence, had we, instead of keeping the English and the French apart, mixed the results of our measurements of them all together? The question is an important one, as it will oblige us to understand more clearly what we mean by homogeneous classes. The answer that would usually be given to it, though substantially correct, is somewhat too decisive and summary. It would be said that we are here mixing distinctly heterogeneous elements, and that in consequence the resultant law of error will be by no means of the simple character previously exhibited. So far as such an answer is to be admitted its grounds are easy to appreciate. In accordance with the usual law of error the divergences from the mean grow continuously less numerous as they increase in amount. Now, if we mix up the French and English heights, what will follow? Beginning from the English mean of 5 feet 9 inches, the heights will at first follow almost entirely the law determined by these English conditions, for at this point the English data are very numerous, and the French by comparison very few. But, as we begin to approach the French mean, the numbers will cease to show that continual diminution which they should, according to the English scale of arrangement, for here the French data are in turn very numerous, and the English by comparison few. The result of such a combination of heterogeneous elements is illustrated by the figure annexed, of course in a very exaggerated form.

---

[1] He scarcely, however, professes to give these as an accurate measure of the mean height, nor does he always give precisely the same measure. Practically, none but soldiers being measured in any great numbers, the English stature does not afford accurate data on any large scale.



§ 14. In the above case the nature of the heterogeneity, and the reasons why it should be avoided, seem tolerably obvious. It will be seen still more plainly if we take a parallel case drawn from artificial proceedings. Suppose that after a man had fired a few thousand shots at a certain spot, say a wafer fixed somewhere on a wall, the position of the spot at which he aims were shifted, and he fired a few thousand more shots at the wafer in its new position. Now let us collect and arrange all the shots of both series in the order of their departure from either of the centres, say the new one. Here we should really be mingling together two discordant sets of elements, either of which, if kept apart from the other, would have been of a simple and homogeneous character. We should find, in consequence, that the resultant law of error betrayed its composite or heterogeneous origin by a glaring departure from the customary form, after the fashion indicated in the last note.

The instance of the English and French heights resembles the one just given, but falls far short of it in the stringency with which the requisite conditions are secured. The fact is we have not here got the necessary requirements, viz. a group consisting of a few fixed causes supplemented by innumerable little disturbing influences. What we call a nation is really a highly artificial body, the members of which are subject to a considerable number of local or occasional disturbing causes. Amongst Frenchmen were included, presumably, Bretons, Provençals, Alsatians, and so on, thus commingling distinctions which, though less than those between French and English, regarded as wholes, are very far from being insignificant. And to these differences of race must be added other disturbances, also highly important, dependent upon varying climate, food and occupation. It is plain, therefore, that whatever objections exist against confusing together French and English statistics, exist also, though of course in a less degree, against confusing together those of the various provincial and other components which make up the French people.

§ 15. Out of the great variety of important causes which influence the height of men, it is probable that those which most nearly fulfil the main conditions required by the 'Law of Error' are those about which we know the least. Upon the effects of food and employment, observation has something to say, but upon the purely physiological causes by which the height of the parents influences the height of the offspring, we have probably nothing which deserves to be called knowledge. Perhaps the best supposition we can make is one which, in accordance with the saying that 'like breeds like', would assume that the purely physiological causes represent the constant element; that is, given a homogeneous race of people to begin with, who freely intermarry, and are subject to like circumstances of climate, food, and occupation, the standard would remain on the whole constant.

In such a case the man who possessed the mean height, mean weight, mean strength, and so on, might then be called, in a sort of way, a 'type'. The deviations from this type would then be produced by innumerable small influences, partly physiological, partly physical and social, acting for the most part independently of one another, and resulting in a Law of Error of the usual description. Under such restrictions and explanations as these, there seems to be no reasonable objection to speaking of a French or English type or mean. But it must always be remembered that under the present circumstances of every political nation, these somewhat heterogeneous bodies might be subdivided into various smaller groups, each of which would also exhibit the characteristics of such a type in an even more marked degree.

It need hardly be added to the above remarks, that no one who gives the slightest adhesion to the Doctrine of Evolution could regard the type, in the above qualified sense of the term, as possessing any real permanence and fixity. If the constant causes, whatever they may be, remain unchanged, and if the variable ones continue in the long run to balance one another, the results will continue to cluster about the same mean. But if the constant ones undergo a gradual change, or if the variable ones, instead of balancing one another suffer one or more of their number to begin to acquire a preponderating influence, so as to put a sort of bias upon their aggregate effect, the mean will at once begin, so to say, to shift its ground. And having once begun to shift, it may continue to do so, to whatever extent we recognize that species are variable and development is a fact. It is as if the point on the target at which we aim, instead of being fixed were slowly changing its position as we continue to fire at it; changing almost certainly to some extent and temporarily, and not improbably to a considerable extent and permanently.

§ 16. Our examples throughout this chapter have been almost exclusively drawn from physical characteristics, whether of man or of inanimate things; but it need not be supposed that we are necessarily confined to such instances. Mr Galton, for instance, has proposed to extend the same principles of calculation to mental phenomena, with a view to their more accurate determination. The objects to be gained by so doing belong rather to the inferential part of our subject, and will be better indicated further on; but they do not involve any distinct principle. Like other attempts to apply the methods of science in the region of the mind, this proposal has met with some opposition; with very partial reason, as it seems to me. That our mental qualities, if they could be submitted to accurate measurement, would be found to follow the usual Law of Error, may be assumed without much hesitation. The known extent of the correlation of mental and bodily characteristics gives high probability to the supposition that what is proved to prevail, at any rate approximately, amongst all bodily elements which have been submitted to measurement, will prevail also amongst the mental elements.

To what extent such *measurements* could be carried out practically, is another matter. It does not seem to me that it could be done with much success; partly because our mental qualities are so closely connected with, indeed so run into one another, that it is impossible to isolate them for purposes of comparison. This is to some extent indeed a difficulty in bodily measurements, but it is incomparably more so in those of the mind. Moreover, even when mental qualities can be separated and distinguished from one another, anything approaching to accurate measurement is out of the question. We can hardly get beyond what can be called a good guess. The doctrine, therefore, that mental qualities follow the now familiar law of arrangement can scarcely be grounded upon anything more than a strong analogy. Still this analogy is quite strong enough to justify us in accepting the doctrine and all the conclusions which follow from it, in so far as our estimates and measurements can be regarded as trustworthy. There seems therefore nothing unreasonable in the attempt to establish a system of natural classification of mankind by arranging them into a certain number of groups above and below the average, each group being intended to correspond to certain limits of excellency or deficiency. All that is necessary for such a purpose is that the rate of departure from the mean should be tolerably constant under widely different circumstances, in this case throughout all the races of men. Of course if the law of divergence is the same as that which prevails in inanimate nature we have a still wider and more natural system of classification at hand, and one which ought to be familiar, more or less, to every one who has thus to estimate qualities.

§ 17. Perhaps one of the best illustrations of the legitimate application of such principles is to be found in Mr Galton's work on *Hereditary Genius*. Indeed the full force and purport of some of his reasonings there can hardly be appreciated except by those who are familiar with the conceptions of this chapter. We can only afford space to notice one or two points, but the student will find in the perusal, of at any rate the more argumentative parts, of that volume[1] an interesting illustration of the doctrines now under discussion. For one thing it may be safely asserted, that no one unfamiliar with the Law of Error would ever in the least appreciate the excessive rapidity with which the superior degrees of excellence tend to become scarce. Every one, of course, can see at once, in a numerical way at least, what is involved in being 'one of a million'; but they would not at all understand, how very little extra superiority is to be looked for in the man who is 'one of two million'. They would confound the mere numerical distinction, which seems in some way to imply double excellence, with the intrinsic superiority, which would mostly be represented by a very small fractional advantage. To be 'one of ten million' sounds very grand, but if the qualities under consideration could be estimated without the knowledge of the vastly wider area from which the selection had been made, and in freedom therefore from any consequent numerical bias, people would be surprised to find what a very slight comparative superiority was, as a rule, thus obtained.

§ 18. The point just mentioned is an important one in arguments from statistics. If, for instance, we find a small group of persons, connected together by blood-relationship, and all possessing some mental characteristic in marked superiority, much depends upon the comparative rarity of such excellence when we are endeavouring to decide whether or not the common possession of these qualities was accidental. Such a decision can never be more than a rough one, but if it is to be made at all this consideration must enter as a factor. Again, when we are comparing one nation with another[1], say the Athenian with any modern European people, does the popular mind at all appreciate what sort of evidence of general superiority is implied by the production, out of one nation, of such a group as Socrates, Plato and so forth? In this latter case we are also, it should be remarked, employing the 'Law of Error' in a second way; for we are assuming that where the extremes are great so will also the means be, in other words we are assuming that every amount of departure from the mean occurs with a (roughly) calculable degree of relative frequency. However generally this truth may be accepted in a vague way, its evidence can only be appreciated by those who know the reasons which can be given in its favour.

---

[1] I refer to the introductory and concluding chapters: the bulk of the book is, from the nature of the case, mainly occupied with statistical and biographical details.

But the same principles will also supply a caution in the case of the last example. They remind us that, for the mere purpose of comparison, the *average* man of any group or class is a much better object for selection than the eminent one. There may be greater difficulties in the way of detecting him, but when we have done so we have got possession of a securer and more stable basis of comparison. He is selected, by the nature of the case, from the most numerous stratum of his society, the eminent man from a thinly occupied stratum; in accordance therefore with the now familiar laws of averages and of large numbers the fluctuations amongst the former will generally be very few and small in comparison with those amongst the latter.

---

[1] See Galton's *Hereditary Genius*, pp. 336—350, "On the comparative worth of different races."

CHAPTER III.

ON THE CAUSAL PROCESS BY WHICH THE SERIES OF PROBABILITY ARE BROUGHT ABOUT.

§ 1. IN discussing the question whether all the series with which Probability is concerned are of precisely one and the same type, we made some examination of the process by which they are naturally produced, but we must now enter a little more into the details of this process. All events are the results of numerous and complicated antecedents, far too numerous and complicated in fact for it to be possible for us to determine or take them all into account. Now, though it is strictly true that we can never determine them all, there is a broad distinction between the case of Induction, in which we can make out enough of them, and with sufficient accuracy, to satisfy a reasonable certainty, and Probability, in which we cannot do so. To Induction we shall return in a future chapter, and therefore no more need be said about it here.

We shall find it convenient to begin with a division which, though not pretending to any great philosophical accuracy, will serve as a preliminary guide. It is the simple division into objects, and the agencies which affect them. All the phenomena with which Probability is concerned (as indeed most of those with which science of any kind is concerned) are the product of certain objects natural and artificial, acting under the influence of certain agencies natural and artificial. In the tossing of a penny, for instance, the objects would be the penny or pence which were successively thrown; the agencies would be the act of throwing, and everything which combined directly or indirectly with this to make any particular face come uppermost. This is a simple and intelligible division, and can easily be so extended in meaning as to embrace every class of objects with which we are concerned.

Now if, in any two or more cases, we had the same object, or objects indistinguishably alike, and if they were exposed to the influence of agencies in all respects precisely alike, we should expect the results to be precisely similar. By one of the applications of the familiar principle of the uniformity of nature we should be confident that exact likeness in the antecedents would be followed by exact likeness in the consequents. If the same penny, or similar pence, were thrown in exactly the same way, we should invariably find that the same face falls uppermost.

§ 2. What we actually find is, of course, very far removed from this. In the case of the objects, when they are artificial constructions, e. g. dice, pence, cards, it is true that they are purposely made as nearly as possible indistinguishably alike. We either use the same thing over and over again, or different ones made according to precisely the same model. But in natural objects nothing of the sort prevails. In fact when we come to examine them, we find reproduced in them precisely the same characteristics as those which present themselves in the final result which we were asked to explain, so that unless we examine them a stage further back, as we shall have to do to some extent at any rate, we seem to be merely postulating again the very peculiarity of the phenomena which we were undertaking to explain. They will be found, for instance, to consist of large classes of objects, throughout all the individual members of which a general resemblance extends. Suppose that we were considering the length of life. The objects here are the human beings, or that selected class of them, whose lives we are considering. The resemblance existing amongst them is to be found in the strength and soundness of their principal organs, together with all the circumstances which collectively make up what we call the goodness of their constitutions. It is true that most of these circumstances do not admit of any approach to actual measurement; but, as was pointed out in the last chapter, very many of the circumstances which do admit of such measurement have been measured, and found to display the characteristics in question. Hence, from the known analogy and correlation between our various organs there can be no reasonable doubt, that if we could arrange human constitutions in general, or the various elements which compose them in particular, in the order of their strength, we should find just such an aggregate regularity and just such groupings about the mean, as the final result (viz. in this case the length of their lives) presents to our notice.

§ 3. It will be observed therefore that for this purpose the existence of natural kinds or groups is necessary. In our games of chance of course the same die may be thrown, or card drawn from the same pack, as often as we please; but many of the events which occur to human beings either cannot be repeated at all, or not often enough to secure in the case of one individual any sufficient statistical uniformity. Such regularity as we trace in nature is owing, much more than is often suspected, to the arrangement of things in natural kinds, each of them containing a large number of individuals. Were each kind of animals or vegetables limited to a single pair, or even to but a few pairs, there would not be much scope left for the collection of statistical tables amongst them. Or to take a less violent supposition, if the numbers in each natural class of objects were much smaller than they are at present, or the differences between their varieties and sub-species much more marked, the consequent difficulty of extracting from them any sufficient length of statistical tables, though not fatal, might be very serious. A large number of objects in the class, together with that general similarity which entitles the objects to be fairly comprised in one class, seem to be important conditions for the applicability of the theory of Probability to any phenomenon. Something analogous to this excessive paucity of objects in a class would be found in the attempt to apply special Insurance offices to the case of those trades where the numbers are very limited, and the employment so dangerous as to put them in a class by themselves. If an Insurance Society were started for the workmen in gunpowder mills alone, a premium would have to be charged to avoid possible ruin, so high as to illustrate the extreme paucity of appropriate statistics.

§ 4. So much (at present) for the objects. If we turn to what we have termed the agencies, we find much the same thing again here. By the adjustment of their relative intensity, and the respective frequency of their occurrence, the total effects which they produce are found to be also tolerably uniform. It is of course conceivable that this should have been otherwise. It might have been found that the second group of conditions so exactly corrected the former as to convert the merely general uniformity into an absolute one; or it might have been found, on the other hand, that the second group should aggravate or disturb the influence of the former to such an extent as to destroy all the uniformity of its effects. Practically neither is the case. The second condition simply varies the details, leaving the uniformity on the whole of precisely the same general description as it was before. Or if the objects were supposed to be absolutely alike, as in the case of successive throws of a penny, it may serve to bring about a uniformity. Analysis will show these agencies to be thus made up of an almost infinite number of different components, but it will detect the same peculiarity that we have so often had occasion to refer to, pervading almost all these components. The proportions in which they are combined will be found to be nearly, though not quite, the same; the intensity with which they act will be nearly though not quite equal. And they will all unite and blend into a more and more perfect harmony as we proceed to take the average of a larger number of instances.

Take, for instance, the length of life. As we have seen, the constitutions of a very large number of persons selected at random will be found to present much the same feature; general uniformity accompanied by individual irregularity. Now when these persons go out into the world, they are exposed to a variety of agencies, the collective influence of which will assign to each the length of life allotted to him. These agencies are of course innumerable, and their mutual interaction complicated beyond all power of analysis to extricate. Each effect becomes in its turn a cause, is interwoven inextricably with an indefinite number of other causes, and reacts upon the final result. Climate, food, clothing, are some of these agencies, or rather comprise aggregate groups of them. The nature of a man's work is also important. One man overworks himself, another follows an unhealthy trade, a third exposes himself to infection, and so on.

Now the result of all this is that these agencies conspire with the objects to reproduce the same kind of uniformity as is mostly to be found in them. Or rather, as we shall proceed presently to show, they do so in the great majority of cases.

§ 5. It may be objected that such an explanation as the above does not really amount to anything deserving of the name, for that instead of explaining how a particular state of things is caused it merely points out that the same state exists elsewhere. There is a uniformity discovered in the objects at the stage when they are commonly submitted to calculation; we then grope about amongst the causes of them, and after all only discover a precisely similar uniformity existing amongst these causes. This is to some extent true, for though part of the objection can be removed, it must always remain the case that the bases of an objective science will rest in the last resort upon the mere fact that things are found to be so.

§ 6. This division, into objects and the agencies which affect them, is merely intended for a rough practical arrangement, sufficient to point out to the reader the immediate nature of the causes which bring about our familiar uniformities. If we go back a step further, it might fairly be maintained that they may be reduced to one, namely to the agencies. The objects, as we have termed them, are not an original creation in the state in which we now find them. No one supposes that whole groups or classes were brought into existence simultaneously, with all their general resemblances and particular differences fully developed. Even if it were the case that the first parents of each natural kind had been specially created, instead of being developed out of pre-existing forms, it would still be true that amongst the numbers of each that now present themselves the characteristic differences and resemblances are the result of what we have termed agencies. Take, for instance, a single characteristic only, say the height; what determines this as we find it in any given group of men? Partly, no doubt, the nature of their own food, clothing, employment, and so on, especially in the earliest years of their life; partly also, very likely, similar conditions and circumstances on the part of their parents at one time or another. No one, I presume, in the present state of knowledge, would attempt to enumerate the remaining causes, or even to give any indication of their exact nature; but at the same time few would entertain any doubt that agencies of this general description have been the determining causes at work.

If it be asked again, Into what may these agencies themselves be ultimately analysed? the answer to this question, in so far as it involves any detailed examination of them, would be quite foreign to the plan of this essay. In so far as any general remarks, applicable to nearly all classes alike of such agencies, are called for, we are led back to the point from which we started in the previous chapter, when we were discussing whether there is necessarily one fixed law according to which all our series are formed. We there saw that every event might be regarded as being brought about by a comparatively few important causes, of the kind which comprises all of which ordinary observation takes any notice, and an indefinitely numerous group of small causes, too numerous, minute, and uncertain in their action for us to be able to estimate them or indeed to take them individually into account at all. The important ones, it is true, may also in turn be themselves conceived to be made up of aggregates of small components, but they are still best regarded as being by comparison simple and distinct, for their component parts act mostly in groups collectively, appearing and disappearing together, so that they possess the essential characteristics of unity.

§ 7. Now, broadly speaking, it appears to me that the requisite conditions for Probability are these: that the important causes should be by comparison fixed and permanent, and that the remaining ones should on the average continue to act as often in one direction as in the other. This they may do in two ways. In the first place we may be able to predicate nothing more of them than the mere fact that they act[1] as often in one direction as the other; what we should then obtain would be merely the simple statistical uniformity that is described in the first chapter. But it may be the case, and in practice generally is so more or less approximately, that these minor causes act also in independence of one another. What we then get is a group of uniformities such as was explained and illustrated in the second chapter. Every possible combination of these causes then occurring with a regular degree of frequency, we find one peculiar kind of uniformity exhibited, not merely in the mere fact of excess and defect (of whatever may be the variable quality in question), but also in every particular amount of excess and defect. Hence, in this case, we get what some writers term a 'mean' or 'type,' instead of a simple average. For instance, suppose a man throwing a quoit at a mark. Here our fixed causes are his strength, the weight of the quoit, and the intention of aiming at a given point. These we must of course suppose to remain unchanged, if we are to obtain any such uniformity as we are seeking. The minor and variable causes are all those innumerable little disturbing influences referred to in the last chapter. It might conceivably be the case that we were only able to ascertain that these acted as often in one direction as in the other; what we should then find was that the quoit tended to fall short of the mark as often as beyond it. But owing to these little causes being mostly independent of one another, and more or less equal in their influence, we find also that every *amount* of excess and defect presents the same general characteristics, and that in a large number of throws the quantity of divergences from the mark, of any given amount, is a tolerably determinate function, according to a regular law, of that amount of divergence[1].

---

[1] As stated above, this is really little more than a re-statement, a stage further back, of the existence of the same kind of uniformity as that which we are called upon to explain in the concrete details presented to us in experience.

§ 8. The necessity of the conditions just hinted at will best be seen by a reference to cases in which any of them happen to be missing. Thus we know that the length of life is on the whole tolerably regular, and so are the numbers of those who die in successive years or centuries of most of the commoner diseases. But it does not seem to be the case with all diseases. What, for instance, of the Sweating Sickness, the Black Death, the Asiatic Cholera? The two former either do not recur, or, if they do, recur in such a mild form as not to deserve the same name. What in fact of any of the diseases which are epidemic rather than endemic? All these have their causes doubtless, and would be produced again by the recurrence of the conditions which caused them before. But some of them do not recur at all. They seem to have depended upon such rare conditions that their occurrence was almost unique. And of those which do recur the course is generally so eccentric and irregular, often so much dependent upon human will or want of will, as to entirely deprive their results (that is, the number of deaths which they cause) of the statistical uniformity of which we are speaking.

---

[1] "It would seem in fact that in coarse and rude observations the errors proceed from a *very few* principal causes, and in consequence our hypothesis [as to the Exponential Law of Error] will probably represent the facts only imperfectly, and the frequency of the errors will only approximate roughly and vaguely to the law which follows from it. But when astronomers, not content with the degree of accuracy they had reached, prosecuted their researches into the remaining sources of error, they found that not three or four, but a *great number* of minor sources of error of nearly co-ordinate importance began to reveal themselves, having been till then masked and overshadowed by the graver errors which had been now approximately removed....... There were errors of graduation, and many others in the contraction of instruments; other errors of their adjustments; errors (technically so called) of *observation;* errors from the changes of temperature, of weather, from slight irregular motions and vibrations; in short, the thousand minute disturbing influences with which modern astronomers are familiar." (Extracted from a paper by Mr Crofton in the Vol. of the *Philosophical Transactions* for 1870, p. 177.)

The explanation probably is that one of the principal causes in such cases is contagion. If so, we have at once a cause which so far from being fixed is subject to the utmost variability. Stringent caution may destroy it, carelessness may aggravate it to any extent. The will of man, as finding its expression either on the part of government, of doctors, or of the public, may make of it pretty nearly what is wished, though against the possibility of its entrance into any community no precautions can absolutely insure us.

§ 9. If it be replied that this want of statistical regularity only arises from the fact of our having confined ourselves to too limited a time, and that we should find it disappear here, as elsewhere, if we kept our tables open long enough, we shall find that the answer will suggest another case in which the requisite conditions for Probability are wanting. Such a reply would only be conclusive upon the supposition that the ways and thoughts of men are in the long run invariable, or if variable, subject to periodic changes only. On the assumption of a steady progress in society, either for the better or the worse, the argument falls to the ground at once. From what we know of the course of the world, these fearful pests of the past may be considered as solitary events in our history, or at least events which will not be repeated. No continued uniformity would therefore be found in the deaths which they occasion, though the registrar's books were kept open for a million years. The reason here is probably to be sought in the gradual alteration of those indefinitely numerous conditions which we term progress or civilization. Every little circumstance of this kind has some bearing upon the liability of any one to catch a disease. But when a kind of slow and steady tide sets in, in consequence of which these influences no longer remain at about the same average strength, warring at equal terms in the long run with hostile influences, but on the contrary show a steady tendency to increase their power, the statistics will, with consequent steadiness and permanence, take the impress of such a change.

§ 10. Briefly then, if we were asked where the distinctive characteristics of Probability are most prominently to be found, and where they are most prominently absent, we might say that (1) they prevail principally in the properties of natural kinds, both in the ultimate and in the derivative or accidental properties. In all the characteristics of natural species, in all they do and all which happens to them, so far as it depends upon their properties, we seldom fail to detect this regularity. Thus in men, their height, strength, weight, the age to which they live, the deaths of which they die; all present a well-known uniformity. Life Insurance tables offer the most familiar instance of the importance of these applications of Probability.

(2) The same peculiarity prevails again in the force and frequency of most natural agencies. Wind and weather are seen to lose their proverbial irregularity when examined on a large scale. Man's work therefore, when operated on by such agencies as these, even though it had been made in different cases absolutely alike to begin with, afterwards shows only a general regularity. I may sow exactly the same amount of seed in my field every year. The yield may one year be moderate, the next abundant through favourable weather, and then again in turn be destroyed by hail. But in the long run these irregularities will be equalized in the result of my crops, because they are equalized in the power and frequency of the productive agencies. The business of underwriters, and offices which insure the crops against hail, would fall under this class; though, as already remarked, there is no very profound distinction between them and the former class.

The reader must be reminded again that this fixity is only temporary, that is, that even here the series belong to the class of those which possess a fluctuating type. Those indeed who believe in the fixity of natural species will have the best chance of finding a series of the really permanent type amongst them, though even they will admit that some change in the characteristic is attainable in length of time. In the case of the principal natural agencies, it is of course incontestable that the present average is referable to the present geological period only. Our average temperature and average rainfall have in former times been totally different from what they now are, and doubtless will be so again.

Any fuller investigation of the process by which on the Theory of Evolution out of a primeval simplicity and uniformity the present variety was educed, hardly belongs to the scope of the present work: at most, a few hints must suffice.

§ 11. The above, then, are instances of natural objects and natural agencies. There seems reason to believe that it is in such things only, as distinguished from things artificial, that the property in question is to be found. This is an assertion that will need some discussion and explanation. Two instances, in apparent opposition, will at once occur to the mind of some readers; one of which, from its great intrinsic importance, and the other, from the frequency of the problems which it furnishes, will demand a few minutes' separate examination.

(1) The first of these is the already mentioned case of instrumental observations. In the use of astronomical and other instruments the utmost possible degree of accuracy is often desired, a degree which cannot be reasonably hoped for by any one single observation. What we do therefore in these cases is to make a large number of different observations, which are naturally found to differ somewhat from one another in their results; by means of these the true value (as explained in a future chapter, on the Method of Least Squares) is to be determined as accurately as possible. The subjects then of calculation here are a certain number of elements, slightly incorrect elements, given by successive observations. Are not these observations artificial, or the direct product of voluntary agency? Certainly not: at least, it depends on what we understand by voluntary. What is really intended and aimed at by the observer is, of course, perfect accuracy, that is, the true observation, or the voluntary steps and preliminaries on which this observation depends. Whether voluntary or not, this result only can be called intentional. But this result is not obtained. What we actually get in its place is a series of deviations from it,

containing results more or less wide of the truth. Now by what are these deviations caused? By just such agencies as we have been considering in some of the earlier sections in this chapter. Heat and its irregular warping influence, draughts of air producing their corresponding effects, dust and consequent friction in one part or another, the slight distortion of the instrument by strains or the slow uneven contraction which continues long after the metal was cast; these and such as these are some of the causes which divert us from the truth. Besides this group, there are others which certainly do depend upon human agency, but which are not, strictly speaking, voluntary. They are such as the irregular action of the muscles, inability to make our various organs and members execute precisely the purposes we have in mind, perhaps different rates in the rapidity of the nervous currents in the same or different observers. The effect produced by some of these, and the allowance that has in consequence to be made, are becoming familiar even to the outside world under the name of the 'personal equation' in astronomical and other observations.

§ 12. (2) The other example, alluded to above, is the stock one of cards and dice. Here, as in the last case, the result is remotely voluntary, in the sense that deliberate volition has come in at one stage. But subsequently to this stage, the result is produced or affected by so many involuntary agencies that it owes its characteristic properties to these. The turning up, for example, of a particular face of a die is the result of voluntary agency, but it is not an immediate result. That particular face was not chosen, though the fact of its being chosen was the remote consequence of an act of choice. There has been an intermediate chaos of conflicting agencies, which no one can calculate before or distinguish afterwards. These agencies seem to show a uniformity in the long run, and thence to produce a similar uniformity in the result. The drawing of a card from a pack is indeed more directly volitional, as in cutting for partners in a game of whist. But no one continues to do this long without having the pack well shuffled in the interval, whereby a host of involuntary influences are let in.

§ 13. The once startling but now familiar uniformities in the cases of suicides and misdirected letters do not belong to the same class. The final resolution, or want of it, is in each case indeed an important ingredient in the individual's action or omission; but, in so far as volition has anything to do with the results *as a whole,* it instantly disturbs them. If the voice of the Legislature speaks out, or any great preacher or moralist succeeds in deterring, or any impressive example in impelling, our moral statistics are instantly tampered with. Some further discussion will be devoted to this subject in a future chapter; it need only be remarked here that (always excluding such common or general influence as those just mentioned) the average volition, potent as it is in each separate case, is on the whole swayed by non-voluntary conditions, such as those of health, the casualties of employment, &c., which influence the length of a man's life.

§ 14. Such distinctions as those just insisted on may seem to some persons to be needless, but serious errors have occasionally arisen from the neglect of them. The immediate products of man's mind, so far as we can obtain them, do not seem to possess this essential characteristic of Probability. Their characteristic seems rather to be, either perfect mathematical accuracy or utter want of it, either law unfailing or mere caprice. If, e.g., we find the trees in a forest growing in straight lines, we unhesitatingly conclude that they were planted by man as they stand. It is true, on the other hand, that if we find them not regularly planted, we cannot conclude that they were not planted by man; partly because the planter may have worked without a plan, partly because the subsequent irregularities brought on by nature may have obscured the plan. Practically the mind has to work by the aid of imperfect instruments, and subjected to many hindrances through various and conflicting agencies, and by these means its work loses its original properties. Suppose, for instance, that a man, instead of producing numerical results by imperfect observations or by the casts of dice, were to select them at first hand for himself by simply thinking of them at once; what sort of series would he obtain? It would be about as difficult to get any such series as those of Probability as it would be to keep his heart or pulse working regularly by direct acts of volition, supposing that he had the requisite control over these organs. But the mere suggestion is absurd. A man must have an object in so thinking, he must think according to a rule or formula; but unless he takes some natural series as a copy, he will never be able to construct one mentally which shall permanently imitate the originals. Or take another product of human efforts, in which the intention can be executed with tolerable success. When any one builds a house there are many slight disturbing influences at work, such as shrinking of bricks and mortar, settling of foundations, &c. But the effect which these disturbances are able to produce is so inappreciably small, that we may fairly consider that the result obtained is the direct product of the mind, the accurate realization of its intention. What is the consequence? Every house in the row is of exactly the same height, width, &c. as its neighbours; or if there are variations they are few, definite, and regular. The result offers no resemblance whatever to the heights, weights, &c. of a number of men selected at random. The builder probably had some regular design in contemplation, and he has succeeded in executing it.

§ 15. It may be replied that if we extend our observation, say to the houses of a large city, we shall then detect the property under discussion. The different heights of a great number, when grouped together, might be found to resemble those of a great number of human beings under similar treatment. Something of this kind might not improbably be found to be the case, though the resemblance would be far from being a close one. But to raise this question is to get on to different ground, for we are speaking (as remarked above) not of the work of different minds with their different aims, but of that of one mind. In a multiplicity of designs there may be that variable uniformity, for which we may look in vain in a single design. The heights which the *different* builders contemplated might be found to group themselves into something of the same kind of uniformity as that which prevails in most other things which they should undertake to do independently. We might then trace the action of the same two conditions,—a uniformity in the multitude of their different designs, a uniformity also in the infinite variety of the influences which have modified those designs. But this is a very different thing from saying that the work of one man will show such a result as this. The difference is much like that between the tread of a thousand men who are stepping without thinking of each other, and their tread when they are drilled into a regiment. In the former case there is the working, in one way or another, of a thousand minds; in the latter, of one only.

The investigations of this and the former chapter constitute a sufficiently close examination into the detailed causes by which the peculiar form of statistical results with which we are concerned is actually produced, to serve the purpose of a work which is occupied mainly with the *method* of the Science of Probability. The great importance, however, of certain statistical or sociological enquiries will demand a recurrence in a future chapter to one particular application of these statistics, viz. to those concerned with some classes of human actions.

§ 16. We will conclude this chapter by a brief discussion of the meaning to be assigned in Probability to certain terms which incessantly occur there, namely, 'randomness' or 'at random,' and other allied expressions. As in the case of all other technical names, their popular signification is no certain clue to that which is accepted in science.

I apprehend that in Probability these characteristics must be regarded as attributes of an agent, either a human agent or one working in an analogous way. This agency is assumed to be fixed and determinate except within certain limits, and within those limits is assumed to be uniformly distributed. It is these two characteristics which are specially distinctive of Probability, and which will therefore most need to be illustrated and explained.

The first characteristic offers nothing remarkable. If the agent does not act in a fixed determinate way, of course he ceases to be a fit object for any kind of science. If he is drawing balls from a bag, for instance, we must assume that he always goes to the same bag, and that its contents are unchanged during his operations; or, if there is any change permitted, its nature and amount must be strictly defined. This is too obvious to need further remark.

Of the two remaining characteristics, the first, namely, the assignment of limits to the randomness, may also seem obvious enough, and so indeed it is in most of the instances which these early chapters will bring to notice. In 'direct probabilities,'[1] as they are termed, where the causes are given, their limits are also mostly given at the same time, or may readily be inferred, but in 'inverse probabilities,'[1] where the causes are not given, very serious confusion may easily arise from this source. What is needed is that we should always know precisely the limits imposed upon this random conduct. In the case of dice and cards we can scarcely go wrong, for these implements are perfectly distinctive, and the rules for their employment mostly unmistakeable. But if a man 'fires a shot at random,' we want a word or two of further definition. Does he fire within a vertical plane, so that only the *distance*, or within a horizontal plane, so that only the *direction*, is variable? If no such specification be given we may still ask whether he is limited downwards by the horizon, i.e. whether he fires both at the ground beneath him as well as into the air above. So in 'drawing a straight line at random in a plane,' to see whether it intersects certain other lines or figures, we want further limits. The line must start from some given point, or it must (in most instances) cross some given figure, so as not to have the infinite scope of the whole plane assigned to it.

§ 17. We now come to the really characteristic point. *Within certain limits the action must be uniformly distributed.* There is a germ of this meaning in the common acceptation of the term, but it has to be much more stringently insisted on in our science; this rigour of meaning is what has already been described as 'idealizing' the randomness. Suppose there are fifty numbered volumes in a shelf, and a man is said to take one of them at random. We start, of course, by requiring that all his movements, at any rate all that affect the result, shall be strictly assigned up to the limit of the given shelf. He goes to the same bookcase, to the same shelf, and so on. But having got him there, we assume that the direction of his hand in taking out a book will be uniformly distributed in the long run along the whole shelf. He is to take one book, ultimately, just as often as another. So if he fires a shot at random, in a given vertical plane, it is the angular direction of the line of fire which is supposed to be uniformly distributed. Every angular deviation is to be equally represented in the long run.

---

[1] For the meaning of these terms, alluded to, see further on, Ch. VI. and for illustrations of the confusion §§ 9—16.

It is here that a number of characteristic ambiguities begin to make themselves felt as a serious perplexity in probability problems. In reference to what elements, or at what stage, is this uniform distribution to occur? If the books in the shelf are not of exactly equal size, is the ultimate uniform selection to apply to the books individually, or to the width, or even to the area, of their backs, so as to make the large books more likely to get taken? Again, in approaching the shelf, does he distribute his movements uniformly over linear or angular spaces? in plainer words, does he walk straight up to any part of the shelf, and take out the book opposite him, or towards the middle and stretch out for one? In the latter case the books at the ends of the shelf would not get their fair share of selection. As we shall see in future chapters, perplexities of this kind impose overwhelming obstacles in the way of certain problems which have been attempted at various times. All that need be noticed here is, that this uniform distribution has to be postulated at one stage or another in all "random" action; and that to determine at what stage it shall be introduced in any particular problem, a variety of conventions exist, founded partly on experience and partly on what may be called instincts of fairness and propriety.

The experience and sagacity of successive ages of gamblers have of course secured that no ambiguity of this kind shall cause them any inconvenience. Dice indeed offer no trouble; for with them the uniform distribution is pretty accurately secured without the need of, and, if necessary in defiance of, any care and forethought. We cannot help getting the successive faces equally often with a fair die. In cutting a pack of cards the same end is secured by first shuffling them, which also is found in the long run to lead to a uniform distribution. And doubtless similar resources are available in any other cases where they may be needed.

CHAPTER IV.

THE MODES OF DISCOVERING AND PROVING THE EXISTENCE OF OUR SERIES.

§ 1. AT the point which we have now reached, we are supposed to be in possession of a series of a certain kind, lying at the bottom, as one may say, and forming the foundation on which the Science of Probability is to be erected. We have described with sufficient particularity the characteristics of such a series, and have indicated the process by which it is, as a rule, actually brought about in nature. The next enquiries which have to be successively made are, how in any particular case we are to proceed to obtain such a series, that is, by what expedients we are to establish its existence and determine its special character and properties? and secondly[1], when we have obtained it, in what mode is it to be employed for logical purposes?

The answer to the former enquiry does not seem difficult. Experience is our sole guide. If we want to discover what is in reality a series of *things*, not a series of our own conceptions, we must appeal to the things themselves to obtain it, for we cannot find much help elsewhere. We cannot tell how many persons will be born or die in a year, or how many houses will be burnt or ships wrecked, without actually counting them. When we thus speak of 'experience' we mean to employ the term in its widest signification; we mean experience supplemented by all the aids which inductive or deductive logic can afford. When, for instance, we have found the series which comprises the numbers of persons of any assigned class who die in successive years, we have no hesitation in extending it some way into the future as well as into the past. The justification of such a procedure must be sought in the ordinary canons of Induction. As a special discussion will be given upon the connection between Probability and Induction, no more need be said upon this subject here; but nothing will be found at variance with the assertion just made, that the series we employ are ultimately obtained by experience only.

---

[1] This latter enquiry belongs to what may be termed the more purely logical part of this volume, and is entered on in the course of Chapter v.

§ 2. In many cases it is undoubtedly true that we do not resort to direct experience at all. If I want to know what is my chance of holding ten trumps in a game of whist, I do not enquire how often such a thing has occurred before. If all the inhabitants of the globe were to divide themselves up into whist parties they would have to keep on at it for a great many years, if they wanted to settle the question satisfactorily in that way. What we do of course is to calculate algebraically the proportion of possible combinations in which ten trumps can occur, and take this as the answer to our problem. So again, if I wanted to know the chance of throwing six with a die whose faces were unequal, I should endeavour to calculate geometrically the solid angle subtended at the centre of gravity by the opposite face, and the ratio of this to the whole surface of a sphere would represent sufficiently closely the chance required.

It is quite true that in such examples as the above, especially the former one, nobody would ever think of appealing to statistics. This would be a tedious process to adopt when, as here, the mechanical and other conditions upon which the production of the events depend are comparatively few, determinate, and admit of isolated consideration, whilst the enormous number of combinations which can be constructed out of them cause an enormous consequent multiplicity of ways in which the events can possibly happen. Hence, in practice, *à priori* determination is often easy, whilst *à posteriori* appeal to experience would be not merely tedious but utterly impracticable. This, combined with the frequent simplicity and attractiveness of such examples when deductively treated, has made them very popular, and produced the impression in many quarters that they are the proper typical instances to illustrate the theory of chance. Whereas, had the science been concerned with those kinds of events only which in practice are commonly made subjects of insurance, probably no other view would ever have been taken than that it was based upon direct appeal to experience.

§ 3. When, however, we look a little closer, we find that there is no occasion for such a sharp distinction as that apparently implied between the two classes of examples just indicated. In such cases as those of dice and cards, even, in which we appear to reason directly from the determining conditions, or possible variety of the events, rather than from actual observation of their occurrence, we shall find that this is only valid by the help of a tacit assumption which can never be determined otherwise than by direct experience. It is, no doubt, an exceedingly natural and obvious assumption, and one which is continually deriving fresh weight from every day observation, but it is one which ought not to be admitted without consideration. As this is a very important matter, not so much in itself as in connection with the light which it throws upon the theory of the subject, we will enter into a somewhat detailed examination of it.

Let us take a very simple example, that of tossing up a penny. Suppose that I am contemplating a succession of two throws; I can see that the only possible events are[1] *H.H.*, *H.T.*, *T.H.*, *T.T.* So much is certain. We are moreover tolerably well convinced from experience that these events occur, in the long run, about equally often. This is of course admitted on all hands. But on the view commonly maintained, it is contended that we might have known the fact beforehand on grounds which are applicable to an indefinite number of other and more complex cases. The form in which this view would generally be advanced is, that we are enabled to state beforehand that the four throws above mentioned are *equally likely*. If in return we ask what is meant by the expression 'equally likely', it appears that there are two and only two possible forms of reply. One of these seeks the explanation in the state of mind of the observer, the other seeks it in some characteristic of the things observed.

(1) It might, for instance, be said on the one hand, that what is meant is that the four events contemplated are equally easy to imagine, or, more accurately, that our expectation or belief in their occurrence is equal. We could hardly be content with this reply, for the further enquiry would immediately be urged, On what ground is this to be believed? What are the characteristics of events of which our expectation is equal? If we consented to give an answer to this further enquiry, we should be led to the second form of reply, to be noticed directly; if we did not consent we should, it seems, be admitting that Probability was only a portion of Psychology, confined therefore to considering states

---

[1] For the use of those not acquainted with the common notation employed in this subject, it may be remarked that *H.H.* is simply an abbreviated way of saying that the two successive throws of the penny give head; *H.T.* that one of them gives head, and the other tail; and so on with the remaining symbols.

of mind in themselves, rather than in their reference to facts viz. as being true or false. We should, that is, be ceasing to make it a science of inference about things. This point will have to be gone into more thoroughly in another chapter; but it is impossible to direct attention too prominently to the fact that logic (and therefore Probability as a branch of logic) is not concerned with what men *do* believe, but with what they ought to believe, if they are to believe correctly.

(2) In the other form of reply the explanation of the phrase in question would be sought, not in a state of mind, but in a quality of the things contemplated. We might give the following as the meaning, viz. that the events really would occur with equal frequency in the long run. The ground of this assertion would probably be found in past experience, and it would doubtless be impossible so to frame the answer as to exclude the notion of our belief altogether. But still there is a broad distinction between seeking an equality in the amount of our belief, as before, and in the frequency of occurrence of the events themselves, as here.

§ 4. When we have got as far as this it can readily be shown that an appeal to experience cannot be long evaded. For *can* the assertion in question (viz., that the throws of the penny will occur equally often) be safely made *à priori?* Those who consider that it can seem hardly to have fully faced the difficulties which meet them. For the moment we begin to enquire seriously whether the penny will really do what is expected of it, we find that restrictions have to be introduced. In the first place it must be an ideal coin, with its sides equal and fair. This restriction is perfectly intelligible; the study of solid geometry enables us to idealize a penny into a circular or cylindrical lamina. But this condition by itself is not sufficient, others are wanted as well. The penny was supposed to be tossed up, as we say 'at random.' What is meant by this, and how is this process to be idealized? To ask this is to introduce no idle subtlety; for it would scarcely be maintained that the heads and tails would get their fair chances if, immediately before the throwing, we were so to place the coin as to start it always with the same side upwards. The difference that would result in consequence, slight as its cause is, would tend in time to show itself in the results. Or, if we persisted in starting with each of the two sides alternately upwards, would the longer repetitions of the same side get their fair chance?

Perhaps it will be replied that if we think nothing whatever about these matters all will come right of its own accord. It may, and doubtless will be so, but this is falling back upon experience. It is here, then, that we find ourselves resting on the experimental assumption above mentioned, and which indeed cannot be avoided. For suppose, lastly, that the circumstances of nature, or my bodily or mental constitution, were such that the same side always *is* started upwards, or indeed that they are started in any arbitrary order of our own? Well, it will be replied, it would not then be a fair trial. If we press in this way for an answer to such enquiries, we shall find that these tacit restrictions are really nothing else than a mode of securing an experimental result. They are only another way of saying, Let a series of actions be performed in such a way as to secure a sequence of a particular kind, viz., of the kind described in the last two chapters.

§ 5. An intermediate way of evading the direct appeal to experience is sometimes found by defining the probability of an event as being measured by the ratio which the number of cases favourable to the event bears to the total number of cases which are possible. This seems a somewhat loose and ambiguous way of speaking. It is clearly not enough to *count* the number of cases merely, they must also be *valued*, since it is not certain that each is equally potent in producing the effect. This, of course, would never be denied, but sufficient importance does not seem to be attached to the fact that we have really no other way of valuing them except by estimating the effects which they actually do, or would produce. Instead of thus appealing to the proportion of cases favourable to the event, it is far better (at least as regards the foundation of the science, for we are not at this moment discussing the practical method of facilitating our calculations) to appeal at once to the proportion of cases in which the event actually occurs.

§ 6. The remarks above made will apply, of course, to most of the other common examples of chance; the throwing of dice, drawing of cards, of balls from bags, &c. In the last case, for instance, one would naturally be inclined to suppose that a ball which had just been put back would thereby have a better chance of coming out again next time, since it will be more in the way for that purpose. How is this to be prevented? If we designedly thrust it to the middle or bottom of the others, we may overdo the precaution; and are in any case introducing human design, that element so essentially hostile to all that we understand by chance. If we were to trust to a good shake setting matters right, we may easily be deceived; for shaking the bag can hardly do more than diminish the disposition of those balls which were already in each other's neighbourhood, to remain there. In the consequent interaction of each upon all, the arrangement in which they start cannot but leave its impress to some extent upon their final positions. In all such cases, therefore, if we scrutinize our language, we shall find that any supposed *à priori* mode of stating a problem is little else than a compendious way of saying, Let means be taken for obtaining a given result. Since it is upon this result that our inferences ultimately rest, it seems simpler and more philosophical to appeal to it at once as the groundwork of our science.

§ 7. Let us again take the instance of the tossing of a penny, and examine it somewhat more minutely, to see what can be actually proved about the results we shall obtain. We are willing to give the pence fair treatment by assuming that they are perfect, that is, that in the long run they show no preference for either head or tail; the question then remains, Will the repetitions of the same face obtain the proportional shares to which they are entitled by the usual interpretations of the theory? Putting then, as before, for the sake of brevity, H for head, and HH for heads twice running, we are brought to this issue;—Given that the chance of H is $\frac{1}{2}$, does it follow necessarily that the chance of HH (with two pence) is $\frac{1}{4}$? To say nothing of 'H ten times' occurring once in 1024 times (with ten pence), need it occur at all? The mathematicians, for the most part, seem to think that this conclusion follows necessarily from first principles; to me it seems to rest upon no more certain evidence than a reasonable extension by Induction.

Taking then the possible results which can be obtained from a pair of pence, what do we find? Four different results may follow, namely, (1) HT, (2) HH, (3) TH, (4) TT. If it can be proved that these four are equally probable, that is, occur equally often, the commonly accepted conclusions will follow, for a precisely similar argument would apply to all the larger numbers.

§ 8. The proof usually advanced makes use of what is called the Principle of Sufficient Reason. It takes this form;—Here are four kinds of throws which may happen;

once admit that the separate elements of them, namely, H and T, happen equally often, and it will follow that the above combinations will also happen equally often, for no reason can be given in favour of one of them that would not equally hold in favour of the others.

To a certain extent we must admit the validity of the principle for the purpose. In the case of the throws given above, it would be valid to prove the equal frequency of (1) and (3) and also of (2) and (4); for there is no difference existing between these pairs except what is introduced by our own notation[1]. TH is the same as HT, except in the order of the occurrence of the symbols H and T, which we do not take into account. But either of the pair (1) and (3) *is* different from either of the pair (2) and (4). Transpose the notation, and there would still remain here a distinction which the mind can recognize. A succession of the same thing twice running is distinguished from the conjunction of two different things by a distinction which does not depend upon our arbitrary notation only, and would remain entirely unaltered by a change in this notation. The principle therefore of Sufficient Reason, if admitted, would only prove that doublets of the two kinds, for example (2) and (4), occur equally often, but it would not prove that they must each occur once in four times. It cannot be proved indeed in this way that they need ever occur at all.

---

[1] I am endeavouring to treat this rule of Sufficient Reason in a way that shall be legitimate in the opinion of those who accept it, but there seem very great doubts whether a contradiction is not involved when we attempt to extract results from it. If the sides are absolutely alike, how can there be any difference between the terms of the series? The succession seems then reduced to a dull uniformity, a mere iteration of the same thing many times; the series we contemplated has disappeared. If the sides are not absolutely alike, what becomes of the applicability of the rule?

§ 9. The formula, then, not being demonstrable *à priori*, (as might have been concluded,) can it be obtained by experience? To a certain extent it can; the present experience of mankind in pence and dice seems to show that the smaller successions of throws do really occur in about the proportions assigned by the theory. But how nearly they do so no one can say, for the amount of time and trouble to be expended before we could feel that we have verified the fact, even for small numbers, is very great, whilst for large numbers it would be simply intolerable. The experiment of throwing often enough to obtain 'heads ten times' has been actually performed by two or three persons, and the results are given by De Morgan, and Mr Jevons[1]. This, however, being only sufficient on the average to give 'heads ten times' a single chance, the evidence is very slight; it would take a considerable number of such experiments to set the matter at rest.

Any such rule, then, as that which we have just been discussing, which professes to describe what will take place in a long succession of throws, is only conclusively proved by experience within very narrow limits, that is, for small repetitions of the same face; within limits less narrow, indeed, we feel assured that the rule cannot be flagrantly in error, otherwise the variation would be almost sure to be detected. From this we feel strongly inclined to infer that the same law will hold throughout. In other words, we are inclined to extend the rule by Induction and Analogy. Still there are so many instances in nature of proposed laws which hold within narrow limits but get egregiously astray when we attempt to push them to great lengths, that we must give at best but a qualified assent to the truth of the formula.

---

[1] Formal Logic, p. 185. Principles of Science, Vol. I. p. 238.

§ 10. The object of the above reasoning is simply to show that we cannot be certain that the rule is true. Let us now turn for a minute to consider the causes by which the succession of heads and tails is produced, and we may perhaps see reasons to make us still more doubtful.

It has been already pointed out that in calculating probabilities *à priori*, as it is called, we are only able to do so by introducing restrictions and suppositions which are in reality equivalent to assuming the expected results. We use words which in strictness mean, Let a given process be performed; but an analysis of our language, and an examination of various tacit suppositions which make themselves felt the moment they are not complied with, soon show that our real meaning is, Let a series of a given kind be obtained; it is to this series only, and not to the conditions of its production, that all our subsequent calculations properly apply. The physical process being performed, we want to know whether anything resembling the contemplated series really will be obtained.

Now if the penny were invariably set the same side uppermost, and thrown with the same velocity of rotation and to the same height, &c.—in a word, subjected to the same conditions,—it would always come down with the same side uppermost. Practically, we know that nothing of this kind occurs, for the individual variations in the results of the throws are endless. Still there will be an *average* of these conditions, about which the throws will be found, as it were, to cluster much more thickly than elsewhere. We should be inclined therefore to infer that if the same side were always set uppermost there would really be a disturbance in the series which we ordinarily look for. In a very large number of throws we should probably begin to find, under such circumstances, that either head or tail was having a preference shown to it. If so, would not similar effects be found to be connected with the way in which we started each successive *pair* of throws? According as we chose to make a practice of putting HH or TT uppermost, might there not be a disturbance in the proportion of successions of two heads or two tails? Following out this train of reasoning, it would seem to point with some likelihood to the conclusion that in order to obtain a series of the kind we expect, we should have to dispose the antecedents in a similar series at the start. The changes and chances produced by the act of throwing might introduce infinite individual variations, and yet there might be found, in the very long run, to be a close similarity between these two series.

§ 11. This is, to a certain extent, only shifting the difficulty, I admit; for the claim formerly advanced about the possibility of proving the proportions of the throws in the former series, will probably now be repeated in favour of those in the latter. Still the question is very much narrowed, for we have reduced it to a series of *voluntary* acts; a man may put whatever side he pleases uppermost. He may act consciously, as I have said, or he may think nothing whatever about the matter, that is, throw at random; if so, it will probably be asserted by many that he will involuntarily produce a series of the kind in question. It may be so, or it may not; it does not seem that there are any easily accessible data by which to decide. All that I am concerned with here is to show the likelihood that the commonly received result does in reality depend upon the fulfilment of a certain condition at the outset, a condition which it is certainly optional with any one to fulfil or not as he pleases. The short successions doubtless will take care of themselves, owing to the infinite complications produced by the casual variations in throwing; but the long ones may suffer, unless their interest be consciously or unconsciously regarded at the outset.

§ 12. The advice, 'Only try long enough, and you will sooner or later get any result that is possible,' is plausible, but it rests only on Induction and Analogy; mathematics do not prove it. As has been repeatedly stated, there are two distinct views of the subject. Either we may, on the one hand, take a series of symbols, call them heads and tails; H, T, &c.; and make the assumption that each of these, and each pair of them, and so on, recurs in the long run with a regulated degree of frequency. We may then calculate their various combinations, and the consequences that may be drawn from the data assumed. This is a purely algebraical process; it is infallible; and there is no limit whatever to the extent to which it may be carried. This way of looking at the matter may be, and undoubtedly should be, nothing more than the counterpart of what I have called the substituted or idealized series which generally has to be introduced as the basis of our calculation. The danger to be guarded against is that of regarding it too purely as an algebraical conception, and thence of sinking into the very natural errors both of too readily evolving it out of our own consciousness, and too freely pushing it to unwarranted lengths.

Or on the other hand, we may consider that we are treating of the behaviour of *things*;—balls, dice, births, deaths, &c.; and drawing inferences about them. But, then, what were in the former instance allowable assumptions, become here propositions to be tested by experience. Now the whole theory of Probability as a practical science, in fact as anything more than an algebraical truth, depends of course upon there being a close correspondence between these two views of the subject, in other words, upon our substituted series being kept in accordance with the actual series. Experience abundantly proves that, between considerable limits, in the example in question, there does exist such a correspondence. But let no one attempt to enforce our assent to every remote deduction that mathematicians can draw from their formulæ. When this is attempted the distinction just traced becomes prominent and important, and we have to choose our side. Either we go over to the mathematics, and so lose all right of discussion about the things; or else we take part with the things, and so defy the mathematics. We do not question the formal accuracy of the latter within their own province, but either we dismiss them as somewhat irrelevant, as applying to data of whose correctness we cannot be certain, or we take the liberty of remodelling them so as to bring them into accordance with facts.

§ 13. A critic of any doctrine can hardly be considered to have done much more than half his duty when he has explained and justified his grounds for objecting to it. It still remains for him to indicate, if only in a few words, what he considers its legitimate functions and position to be, for it can seldom happen that he regards it as absolutely worthless or unmeaning. I should say, then, that when Probability is thus divorced from direct reference to objects, as it substantially is by not being founded upon experience, it simply resolves itself into the common algebraical or arithmetical doctrine of Permutations and Combinations. The considerations upon which these depend are purely formal and necessary, and can be fully reasoned out without any appeal to experience. We there start from pure considerations of number or magnitude, and we terminate with them, having only arithmetical calculations to connect them together. I wish, for instance, to find the chance of throwing heads three times running with a penny. All I have to do is first to ascertain the possible number of throws. Permutations tell me that with two things thus in question (viz. head and tail) and three times to perform the process, there are eight possible forms of the result. Of these eight one only being favourable, the chance in question is pronounced to be one-eighth.

Now though it is quite true that the actual calculation of every chance problem must be of the above character, viz. an algebraical or arithmetical process, yet there is, it seems to me, a broad and important distinction between a material science which employs mathematics, and a formal one which consists of nothing but mathematics. When we cut ourselves off from the necessity of any appeal to experience, we are retaining only the intermediate or calculating part of the investigation; we may talk of dice, or pence, or cards, but these are really only names we choose to give to our symbols. The H's and T's with which we deal have no bearing on objective occurrences, but are just like the x's and y's with which the rest of algebra deals. Probability in fact, when so treated, seems to be absolutely nothing else than a system of applied Permutations and Combinations.

It will now readily be seen how narrow is the range of cases to which any purely deductive method of treatment can apply. It is almost entirely confined to such employments as games of chance, and, as already pointed out, can only be regarded as really trustworthy even there, by the help of various tacit restrictions. This alone would be conclusive against the theory of the subject being rested upon such a basis. The experimental method, on the other hand, is, in the same theoretical sense, of universal application. It would include the ordinary problems furnished by games of chance, as well as those where the dice are loaded and the pence are not perfect, and also the indefinitely numerous applications of statistics to the various kinds of social phenomena.

§ 14. The particular view of the deductive character of Probability above discussed, could scarcely have intruded itself into any other examples than those of the nature of games of chance, in which the conditions of occurrence are by comparison few and simple, and are amenable to accurate numerical determination. But a doctrine, which is in reality little else than the same theory in a slightly disguised form, is very prevalent, and has been applied to truths of the most purely empirical character. This doctrine will be best introduced by a quotation from Laplace. After speaking of the irregularity and uncertainty of nature as it appears at first sight, he goes on to remark that when we look closer we begin to detect "a striking regularity which seems to suggest a design, and which some have considered a proof of Providence. But, on reflection, it is soon perceived that this regularity is nothing but the development of the respective probabilities of the simple events, which ought to occur more frequently according as they are more probable[1]."

If this remark had been made about the succession of heads and tails in the throwing up of a penny, it would have been intelligible. It would simply mean this: that the constitution of the body was such that we could anticipate with some confidence what the result would be when it was treated in a certain way, and that experience would justify our anticipation in the long run. But applied as it is in a more general form to the facts of nature, it seems really to have but little meaning in it. Let us test it by an instance. Amidst the irregularity of individual births, we find that the male children are to the female, in the long run, in about the proportion of 106 to 100. Now if we were told that there is nothing in this but "the development of their respective probabilities," would there be anything in such a statement but a somewhat pretentious re-statement of the fact already asserted? The probability *is* nothing but that proportion, and is unquestionably in this case derived from no other source but the statistics themselves; in the above remark the attempt seems to be made to invert this process, and to derive the sequence of events from the mere numerical statement of the way in which they occur.

---

[1] *Essai Philosophique.* Ed. 1825, p. 74.

§ 15. It will very likely be replied that by the probability above mentioned is meant, not the mere numerical proportion between the births, but some fact in our constitution upon which this proportion depends; that just as there was a relation of equality between the two sides of the penny, which produced the ultimate equality in the number of heads and tails, so there may be something in our constitution or circumstances in the proportion of 106 to 100, which produces the observed statistical result. When this something, whatever it might be, was discovered, the observed numbers might be supposed capable of being determined beforehand. Even if this were the case, however, it must not be forgotten that there could hardly fail to be, in combination with such causes, other concurrent conditions in order to produce the ultimate result; just as besides the shape of the penny, we had also to take into account the nature of the 'randomness' with which it was tossed. What these may be, no one at present can undertake to say, for the best physiologists seem indisposed to hazard even a guess upon the subject[1]. But without going into particulars, one may assert with some confidence that these conditions cannot well be altogether independent of the health, circumstances, manners and customs, &c. (to express oneself in the vaguest way) of the parents; and if once these influencing elements are introduced, even as very minute factors, the results cease to be dependent only on fixed and permanent conditions. We are at once letting in other conditions, which, if they also possess the characteristics that distinguish Probability (an exceedingly questionable assumption), must have that fact specially proved about them. That this should be the case indeed seems not merely questionable, but almost certainly impossible; for these conditions partaking of the nature of what we term generally, Progress and Civilization, cannot be expected to show any permanent disposition to hover about an average.

---

[1] An opinion prevailed rather at one time (quoted and supported by Quetelet amongst others) that the relative ages of the parents had something to do with the sex of the offspring. If this were so, it would quite bear out the above remarks. As a matter of fact, it should be

§ 16. The reader who is familiar with Probability is of course acquainted with the celebrated theorem of James Bernoulli. This theorem, of which the examples just adduced are merely particular cases, is generally expressed somewhat as follows:—that in the long run all events will tend to occur with a frequency proportional to their objective probabilities. With the mathematical proof of this theorem we need not trouble ourselves, as it lies outside the province of this work; but indeed if there is any value in the foregoing criticism, the basis on which the mathematics rest is faulty, owing to there being really nothing which we can with propriety call an objective probability.

If one might judge by the interpretation and uses to which this theorem is sometimes exposed, we should regard it as one of the last remaining relics of Realism, which after being banished elsewhere still manages to linger in the remote province of Probability. It would be an illustration of

observed, that the proportion of 106 to 100 does not seem by any means universal in all countries or at all times. For various statistical tables on the subject see Quetelet, *Physique Sociale*, Vol. I. 166, 173, 238.

the inveterate tendency to objectify our conceptions, even in cases where the conceptions had no right to exist at all. A uniformity is observed; sometimes, as in games of chance, it is found to be so connected with the physical constitution of the bodies employed as to be capable of being inferred beforehand; though even here the connection is by no means so necessary as is commonly supposed, owing to the fact that in addition to these bodies themselves we have also to take into account their relation to the agencies which influence them. This constitution is then converted into an 'objective probability', supposed to develope into the sequence which exhibits the uniformity. Finally, this very questionable objective probability is assumed to exist, with the same faculty of development, in all the cases in which uniformity is observed, however little resemblance there may be between these and games of chance.

§ 17. How utterly inappropriate any such conception is to most of the cases in which we find statistical uniformity, will be obvious on a moment's consideration. The observed phenomena are generally the product, in these cases, of very numerous and complicated antecedents. The number of crimes, for instance, annually committed in any society, is a function amongst other things, of the strictness of the law, the morality of the people, their social condition, and the vigilance of the police, each of these elements being in itself almost infinitely complex. Now, as a result of all these agencies, there is some degree of uniformity; but what has been called above the change of type, which it sooner or later tends to display, is unmistakeable. The average annual numbers do not show a steady gradual approach towards what might be considered in some sense a limiting value, but, on the contrary, fluctuate in a way which, however it may depend upon causes, shows none of the permanent uniformity which is characteristic of games of chance. This fact, combined with the obvious arbitrariness of singling out, from amongst the many and various antecedents which produced the observed regularity, a few only, which should constitute the objective probability (if we took all, the events being absolutely determined, there would be no occasion for an appeal to probability in the case), would have been sufficient to prevent any one from assuming the existence of any such thing, unless the mistaken analogy of other cases had predisposed him to seek for it. This, it appears to me, is the theoretical objection to such a theorem as that of Bernoulli, which seems merely the exploded doctrine of an ideal something which is perpetually striving, and gradually, though never perfectly, succeeding in realising itself in nature. It therefore presupposes a fixed type towards which we might approximate for ever without any fear of overstepping our due limits. This, however, is a state of things which, if we insist upon rigid accuracy, nature probably nowhere presents, and which in many departments she is very far indeed from presenting at all.

There is a familiar practical form of the same error, the tendency to which may not improbably be derived from a similar theoretical source. It is that of continuing to accumulate our statistical data to an almost indefinite extent of time or space. If the type were absolutely fixed we could not possibly have too many statistics; the longer we chose to take the trouble of collecting them the more accurate our results would be. But if the type is changing, in other words, if some of the principal causes which aid in their production have, in regard to their present degree of intensity, strict limits of time or space, we shall do harm rather than good if we overstep these limits. The danger of stopping too soon is easily seen, but in avoiding it we must not fall into the opposite error of going on too long, and so getting either gradually or suddenly under the influence of a changed set of circumstances.

§ 18. This chapter was intended to be devoted to a consideration, not of the processes by which nature produces the series with which we are concerned, but of the theoretic basis of the methods by which we can determine the existence of such series. But it is not possible to keep the two enquiries apart, for here, at any rate, the old maxim prevails that to know a thing we must know its causes. Recur for a minute to the considerations of the last chapter. We there saw that there was a large class of events, the conditions of production of which could be said to consist of (1) a comparatively few nearly unchangeable elements, and (2) a vast number of independent and very changeable elements. At least if there were any other elements besides these, we are assumed either to make special allowance for them, or to omit them from our enquiry. Now in certain cases, such as games of chance, the unchangeable elements may without practical error be regarded as really unchangeable throughout any range of time and space. Hence, as a result, the deductive method of treatment becomes in their case at once the most simple, natural, and conclusive; but, as a further consequence, the statistics of the events, if we choose to appeal to them, may be collected *ad libitum* with better and better approximation to truth. On the other hand, in all social applications of Probability, the unchangeable causes can only be regarded as really unchangeable under many qualifications. We know little or nothing of them directly; they are often in reality numerous, indeterminate, and fluctuating; and it is only under the guarantee of stringent restrictions of time and place, that we can with any safety attribute to them sufficient fixity to justify our theory. Hence, as a result, the deductive method, under whatever name it go, becomes totally inapplicable both in theory and practice; and, as a further consequence, the appeal to statistics has to be made with the caution in mind that we shall do mischief rather than good if we go on collecting too many of them.

§ 19. The results of the last two chapters may be summed up as follows:—We have extended the conception of a series obtained in the first chapter; for we have found that these series are mostly presented to us in groups. These groups are found upon examination to be formed upon approximately the same type throughout a very wide and varied range of experience; the causes of this agreement we discussed and explained in some detail. When, however, we extend our examination by supposing the series to run to a very great length, we find that they may be divided into two classes separated by important distinctions. In one of these classes (that containing the results of games of chance) the conditions of production, and consequently the laws of statistical occurrence, may be practically regarded as absolutely fixed; and the extent of the divergences from the mean seem to know no finite limit. In the other class, on the contrary (containing the bulk of ordinary statistical enquiries), the conditions of production vary with more or less rapidity, and so in consequence do the results. Here variations from the mean are often impossible when exceeding a certain amount. The former we may term *ideal* series. It is they alone which show the requisite characteristics with any close approach to accuracy, and to make the theory of the subject tenable, we have really to substitute one of this kind for one of the less perfect ones of the other class when these latter are under treatment. The former class have, however, been too exclusively considered by writers on the subject; and conceptions appropriate only to them, and not always even to them, have been imported into the other class. It is in this way that semi-realistic doctrines have been introduced, and a general tendency to an excessive deductive or *à priori* treatment of the science has been encouraged.

ON AVERAGES.

§ 20. We have already spoken about averages and means, and the distinction which it has been attempted to draw between them; but the importance of the subject and the many references to it which occur in the theory and practice of Probability, will now demand for it a somewhat more detailed investigation.

I maintain, then, that the average[1], by whatever name it may go, is in all cases one and the same thing; and that any distinctions with which the difference of name is connected cannot be satisfactorily regarded as depending upon the nature of the average itself, i.e. upon the essential process of obtaining it, but either (1) upon certain characteristics of the things from which it is obtained, or (2) upon the purposes which it is intended to subserve.

The average or mean is a fictitious unity, substituted for a plurality of actual given magnitudes. It is obtained by adding together the given magnitudes, and dividing the total by their number. This is the only certain arithmetical way of obtaining it which is equally available in all cases, as e.g. when the magnitudes in question are connected by no known law. Of course if they are connected by a law the data afforded by a few cases may be sufficient to enable us to infer the rest in accordance with this law, and hence the process of obtaining the average may be much abbreviated. Mr Galton has given, for instance, some ingenious suggestions for obtaining the average height of a number of men without the trouble and risk of separately measuring them all. By the eye alone, provided all the objects can be brought under inspection at once, or in successive groups, we can often say of any given two which is the tallest, and therefore we can arrange them in order without the necessity of applying a measure. "A barbarian chief might often be induced to marshal his men in the order of their heights, or in that of the popular estimate of their skill in any capacity; but it would require some apparatus and a great deal of time to measure each man separately, even supposing it possible to overcome the usually strong repugnance of uncivilized people to any such proceeding[1]." It being inductively known (as was explained at some length in Ch. II.) that the heights of any homogeneous race of men are apt to group themselves symmetrically according to a well-known law, the middle man when the set are arranged as above, will represent the mean, and may be subjected to measurement at our ease.

---

[1] We are talking here, of course, of the Arithmetical average or mean, as the most usual and important one; indeed almost the only one with which we need be concerned. But most of the above remarks would apply equally to the geometrical or harmonical mean.

The above suggestion has, of course, nothing to do with the nature of an average, but only refers to the practical process of obtaining it in certain cases.

§ 21. It must be clearly borne in mind that the average is a result of our own processes; it has no necessary connection with the modes in which nature produces the magnitudes so treated. Moreover it concerns a finite number of magnitudes, whilst the magnitudes from which this finite selection is made may be indefinitely numerous. This latter consideration will supply an answer to a possible objection. It was pointed out at some length in Chh. I. and II. that it was the characteristic of the things with which probability is concerned to present in the long run a continually intensifying uniformity. Now a conceivable objection may be raised against regarding the above-mentioned arrangement of things by which order emerges out of disorder as deserving any special notice, on the ground that from the nature of an arithmetical average it could not possibly be otherwise. The process by which an average is obtained, it may be urged, insures this tendency to equalization whatever might be the properties or the actual arrangement of the things themselves. For instance, let there be a party of ten men, of whom four are tall and four are short, and take the average height of any five of them. Since this number cannot be made up of tall men only, or short men only, it stands to reason that the averages cannot differ so much amongst themselves as the single measures may. Is not then the equalizing process, it may be asked, which is observable on increasing the range of our observations, one which can be shown to follow from necessary laws of arithmetic, and one therefore which might be asserted *à priori?*

---

[1] *Philosophical Magazine,* Jan. 1875.

Whatever apparent force there may be in the above objection arises principally from the arbitrary limitations in the particular example selected, in which the number chosen was so large a proportion of the total as to exclude the bare possibility of only extreme cases being contained within it. Let us take an instance in which the number selected is by comparison smaller: suppose a long succession of deaths, and examine the average duration of any ten of the lives thus expiring. No conclusive reason could have been assigned why any ten infants should not all live to the age of 100, or all die one year after they were born. We can simply state the fact, discovered and proved by experience, that such averages are more nearly equal than the individual lives which compose them. It is doubtless quite true that if we take a given limited number of examples, and select groups out of them, we shall find that the averages of these groups present far more uniformity than the single examples of which these averages are composed. If the numbers in each group bear any considerable proportion to the whole, this is necessarily the case. But it is quite a different thing to make the same assertion when the groups do not bear more than a small proportion to the series out of which they are taken, still more so when the numbers in the series, from which the selection is made, are practically or really unlimited. The average height of any of the different batches of fifty men that might be selected from a party of seventy must be tolerably nearly equal; but there is no such necessity when a batch of fifty is taken from 1000, still less when it is selected from 20,000,000. This latter is not unfrequently the characteristic of things as they present themselves in nature, and it is this which is asserted to be solely established by experience.

§ 22. It was said, a few lines back, that no conclusive reason can be given why any ten infants should not all live to 100, or all die when one year old. To this it may be replied, that the intermediate values for the average are more probable than the extreme ones, on the ground that there are so many more ways in which they could be brought about. This, it may be said, follows from the principles of Combinations. An average life of *one* year could only have been brought about (fractions of years being omitted from consideration) in a very few ways, viz. by all, or most of them, dying at the earliest age; and the same would hold with regard to an average age of 100 years, supposing this to be the extreme limit in the other direction. Whereas the intermediate values could have been brought about by a vastly greater variety of combinations of different ages. This is quite true, but the difficulty has been discussed and answered already. How do we know that cases which have the most numerous causes, that is, which can be brought about in the greatest variety of different ways, will in consequence occur with most frequency, except upon grounds of experience? In other words (to use the usual technical phrase), how do we know that the causes which produce the events in question are strictly speaking '*independent*'?

§ 23. When it was said above that the average is a fictitious quantity, all that was meant is that there is not necessarily any magnitude, amongst the things whose average is taken, precisely corresponding to it. In the case of an average of a small number of magnitudes, especially of course when they are taken at random, that is, when they are known or suspected to be connected by no regular law, this is too obvious to need justification. Take an average of the height of a dozen people at a dinner-table, and it is unlikely that this average shall correspond exactly (let us say within the tenth of an inch) with the actual height of any one of the party.

The sort of instances, however, which Quetelet and some others have in view, and to which they wish to apply the term 'mean', in distinction from average, are not such limited selections as those just mentioned, but rather those interminable natural, or quasi natural series, of which we have repeatedly spoken as presenting themselves in *groups*. They maintain that in these cases the mean represents an actually existing thing, whereas in the case of finite and capricious selections of things, like those mentioned above, it is a purely fictitious creation of our own. Much of what might be urged on this subject has been already advanced in discussing the supposed existence of human or other 'types', but a few remarks may conveniently be added here.

Let the reader recur to the three principal classes of things mentioned in Ch. II. § 4. The first of these consisted of the results of games of chance. Toss a die up ten times; the total number of pips on the upper sides may be anything from ten up to sixty. Suppose it to be thirty. We then say that the average number on these occasions is three. Take another set of ten throws, and we may get another average, say four. There is clearly nothing objective or real corresponding to these averages. No doubt if we go on long enough we shall find our averages[1] centreing about three and a half; we call this *the* average throw, or the 'probable' number of points; and this ultimate average or probable throw might have been reasonably asserted beforehand from the constitution of the body; but it has no other truth or reality about it of the nature of a type. It is simply the limit towards which the averages tend; it may happen to be exactly obtained on various particular occasions now and then, but that is all.

The next class is that afforded by the members of any natural group of objects. Much the same remarks may be repeated here. There is very frequently a 'limit' towards which the averages of increasing numbers of individuals tend; and it is of course quite possible that any one of these averages may hit that limit exactly, just as any one of the individuals might have done; but we must not expect this to happen often. When this does occur, and the limit is regarded in the light of a type to which all were meant to attain, we should speak of any such individual as one of the rare specimens exactly corresponding to their type.

---

[1] By this nothing more is asserted of these averages than could have been asserted of the original individual figures; they too 'centre' about the same point, though with less concentration. This is an important point in all applications of averages taken with a view to secure accuracy, as for instance in the method of Least Squares; it is further discussed in the chapter devoted to that subject.

The remaining, or third, class stands of course on a somewhat different ground. When we make a succession of imperfect attempts of any kind, we get a corresponding series of deviations from the mark aimed at. These we may treat arithmetically, and obtain averages, just as in the preceding cases; and most of what was said about the former will apply equally to these latter. The averages are fictions which may or may not happen to hit the truth; generally not. But although these averages (except the lucky exceptions) have nothing objectively true corresponding to them, their *limit* (in a sense) has, for this limit coincides with the fixed object aimed at.

§ 24. On the whole then there seems no sufficient reason for drawing any distinction between averages and means. When we want to call attention to such important points as (1) the distinction between taking averages of a finite capriciously selected body of things, and averages of groups from such series as the three above mentioned, and (2) the fact that in the case of one of these latter series there is an objective reality corresponding not indeed necessarily to any one of these averages, but rather to their ultimate limit, it is best to state expressly what we have in view. It does not seem advisable to indicate it indirectly by applying different names to the averages, which in themselves are in all cases alike essentially the same. When we wish to speak of the 'limit', or to imply its existence, it is better to do so under this name, than to do so indirectly by asserting that the things in question have a mean.

§ 25. Averages are taken for various purposes. Sometimes we substitute an average for the individual details from which it is taken, merely to save trouble, and because we do not need to be accurate. If I say that the average height of Englishmen is five inches more than that of Frenchmen, I may merely wish to indicate the fact that we shall oftener find ourselves right than wrong by assuming one of the former race to be taller than one of the latter. It is a compendious way of stating facts, the full expression of which would be tedious and inconvenient.

Mainly, however, the average is used as a means of attaining accuracy: it may subserve this either (1) statistically or (2) causally. I. In each of the three classes of series mentioned above when we want to know the 'limit,' and are unable to obtain it deductively, we take the average of a number small or great. We cannot hope thus to secure the limit, except by a lucky accident; but, as may be judged from what has been said, and as will be fully explained when we come to treat of the method of Least Squares, we more often and more nearly secure it by an average of many than by one of few, and by an average than by the single results. II. In the other case the use of an average is to aid in finding the cause of a phenomenon. In any single event the action of the fixed causes is interfered with by that of numerous irregular ones. But in the *average* of a number we may assume that these irregular causes will balance one another, and therefore disappear. We cannot hope that they will do this accurately except in the limit, but at any rate they more often and more nearly do so in an average, and therefore we prefer to adopt this plan. Descriptions of the methods thus adopted, with all needful particularity, will be found in works specially devoted to the principles and methods of experimental science. See e.g. Whewell, *Novum Organon Renovatum*, Book III. ch. vii., and W. S. Jevons, *Principles of Science*, Book III. chh. xv.—xvii.

§ 26. Two technical terms may conveniently be explained here; 'mean', or 'average', 'error', and 'probable error'.

The mean or average error is simple and obvious enough. It is merely the average magnitude of the errors[1]. In just the same way as we take an average of a given number of magnitudes, so may we take the average of the departures of these magnitudes from their limit or ultimate average, their 'errors' as they are termed.

The probable error is a somewhat misleading term. By this is meant that particular error which, when the magnitudes are symmetrically arranged as described in Ch. II., just divides those on one side of the mean into two halves in respect of the frequency of their occurrence. This is, in other words, the same thing as to say that the probable error is that which just as many of the errors in the long run exceed as fall short of; so that we are equally likely in any given case to have a result better than that or worse than that.

In thus speaking of errors, the reader may be reminded again of the extremely general signification which has to be attached to this term. It is not confined to human attempts at measurement and the consequent deviations from the truth, but is equally applied to any results, natural or artificial, which show the same tendency to cluster about a mean.

§ 27. It must be remarked that, in order to know what the value of this mean error and probable error really are, or indeed to know the value of any individual error, it is presupposed that we know the limit or ultimate average. This is a knowledge which we are very far from always possessing[1]. Recurring once more to the three principal classes of events with which we are concerned, we can readily see that in the case of games of chance we mostly do possess this knowledge. Instead of appealing to experience to ascertain the limit, we practically deduce it by simple mechanical or arithmetical considerations, and then the 'error' in any individual case or group of cases is obviously found by comparing the results thus obtained with that which theory informs us would ultimately be obtained in the long run. In the case of deliberate efforts at an aim (the third class) we may or may not know accurately the value or position of this aim. In astronomical observations we do not know it, and the method of Least Squares is a method for helping us to ascertain it as well as we can; in such experimental results as firing at a mark of course we do know it, and may thus test the nature and amount of our failure by direct experience. In the remaining case, namely that of what we have termed natural kinds or groups of things, not only do we not know the ultimate limit, but its existence is always at least doubtful, and in many cases may be confidently denied. Where it does exist, that is, where the type seems for all practical purposes permanently fixed, we can only ascertain it by a laborious resort to statistics. Having done this, we may then test by it the results of observations on a small scale. For instance, we find that the ultimate proportion of male to female births is about 106 to 100, we may then compare the statistics of some particular district or town and speak of the consequent 'error', viz. the departure, in that particular and special district, from the general average.

---

[1] Average magnitude, that is, irrespective of the fact of their being in excess or in defect, or, as it is expressed algebraically, of their *sign*. Two equal errors, of which one was positive and the other negative, added algebraically would cancel each other, and so leave no error. But when, as above, we are merely taking their actual magnitude into account they must be equally reckoned.

The probable error and the mean error may occasionally happen to coincide, but they will not often do so. An extreme instance will show how far this may be from being the case. Suppose a regiment of soldiers going into a deadly campaign which will last one year. Half of the men, say, will be killed during it; the others are then disbanded, and being picked men will have long lives. Now their probable duration of life as above defined (for this corresponds to the 'errors' in the definition) is one year; that is, half of them live one year or less, the other half more than that. But their mean duration of life will be much more, for in estimating this the *length* of the surviving lives is taken into account as well as their mere number. If these surviving lives have an average of fifty years, the mean duration of life will be somewhat more than twenty-five years.

---

[1] For all practical purposes, however, the average of a moderate number so nearly corresponds with its ultimate value or limit, that we shall go very little wrong by substituting the former for the latter.

CHAPTER V.

THE SUBJECTIVE SIDE OF PROBABILITY. GRADATIONS OF BELIEF.

§ 1. HAVING now obtained a clear conception of a certain kind of series, the next enquiry is, What is to be done with this series? How is it to be employed as a means of making inferences? The general step that we are now about to take might be described as one from the objective to the subjective, from the things themselves to the state of our minds in contemplating them.

The reader should observe that a substitution has, in a great number of cases, already been made as a first stage towards bringing the things into a shape fit for calculation. This substitution, as described in former chapters, is, in a measure, a process of *idealization.* The series we actually meet with are apt to show a changeable type, and the individuals of them will sometimes transgress their licensed irregularity. Hence they have to be pruned a little into shape, as natural objects almost always have before they are capable of being accurately reasoned about. The form in which the series emerges is that of a series with a fixed type, and with its unwarranted irregularities omitted. This imaginary or ideal series is the basis of our calculation.

It must not be supposed that this is at all at variance with the assertion previously made, that Probability is a science of inference about real things; it is only by a substitution of the above kind that we are enabled to reason about the things. In nature nearly all phenomena present themselves in a form which departs from that rigorously accurate one which scientific purposes mostly demand, so we have to introduce an imaginary series, which shall be free from any such defects. The only condition to be fulfilled is, that the substitution is to be as little arbitrary, that is, to vary from the truth as slightly as possible. This kind of substitution generally passes without notice when natural objects of any kind are made subjects of exact science. I direct distinct attention to it here simply from the apprehension that want of familiarity with the subject-matter might lead some readers to suppose that it involves, in this case, an exceptional deflection from accuracy in the formal process of inference.

It may be remarked also that the adoption of this imaginary series offers no countenance whatever to the doctrine criticised in the last chapter, in accordance with which it was supposed that our series possessed a fixed unchangeable type which was merely the "development of the probabilities" of things, to use Laplace's expression. It differs from anything contemplated on that hypothesis by the fact that it is to be recognized as a necessary substitution of our own for the actual series, and to be kept in as close conformity with facts as possible. It is a mere fiction or artifice necessarily resorted to for the purpose of calculation, and for this purpose only.

This caution is the more necessary, because in the example that I shall select, and which belongs to the most favourite class of examples in this subject, the substitution becomes accidentally unnecessary. The things, as has been repeatedly pointed out, may sometimes need no trimming, because in the form in which they actually present themselves they *are* almost idealized. In most cases a good deal of alteration is necessary to bring the series into shape, but in some—prominently in the case of games of chance—we find the alterations, for all practical purposes, needless.

§ 2. We start then, from such a series as this, upon the enquiry, What kind of inference can be made about it? It may assist the logical reader to inform him that our first step will be analogous to one class of what are commonly known as *immediate* inferences,—inferences, that is, of the type,—'All men are mortal, therefore any particular man or men are mortal.' This case, simple and obvious as it is in Logic requires very careful consideration in Probability.

It is obvious that we must be prepared to form an opinion upon the propriety of taking the step involved in making such an inference. Hitherto we have had as little to do as possible with the irregular individuals; we have regarded them simply as fragments of a regular series. But we cannot long continue to neglect all consideration of them. Even if these events in the gross be tolerably certain, it is not only in the gross that we have to deal with them; they constantly come before us a few at a time, or even as individuals, and we have to form some opinion about them in this state. An Insurance Office, for instance, deals with numbers large enough to obviate most of the uncertainty, but each of their transactions has another party interested in it—What has the man who insures to say to their proceedings? for to him this question becomes an individual one. And even the Office itself receives its cases singly, and would therefore like to have as clear views as possible about these single cases. Now, the remarks made in the preceding chapters about the subjects which Probability discusses might seem to preclude all enquiries of this kind, for was not ignorance of the individual presupposed to such an extent that even (as will be seen hereafter) causation might be denied, within considerable limits, without affecting our conclusions? The answer to this enquiry will require us to turn now to the consideration of a totally distinct side of the question, and one which has not yet come before us. Our best introduction to it will be by the discussion of a special example.

§ 3. Let a penny be tossed up a very great many times; we may then be supposed to know for certain this fact (amongst many others) that in the long run head and tail will occur about equally often. But suppose we consider only a moderate number of throws, or fewer still, and so continue limiting the number until we come down to three or two, or even one? We have, as the extreme cases, certainty or something undistinguishably near it, and utter uncertainty. Have we not, between these extremes, all gradations of belief? There is a large body of writers, including some of the most eminent authorities upon this subject, who state or imply that we are distinctly conscious of such a variation of the amount of our belief, and that this state of our minds can be measured and determined with almost the same accuracy as the external events to which they refer. The principal mathematical supporter of this view is De Morgan, who has insisted strongly upon it in all his works on the subject. The clearest exposition of his opinions will be found in his *Formal Logic*, in which work he has made the view which we are now discussing the basis of his system. He holds that we have a certain amount of belief of every proposition which may be set before us, an amount which in its nature admits of determination, though we may practically find it difficult in any particular case to determine it. He considers, in fact, that Probability is a sort of sister science to Formal Logic[1], speaking of it in the following words: "I cannot understand why the study of the effect, which partial belief of the premises produces with respect to the conclusion, should be separated from that of the consequences of supposing the former to be absolutely true"[1]. In other words, there is a science—Formal Logic—which investigates the rules according to which one proposition can be necessarily inferred from another; in close correspondence with this there is a science which investigates the rules according to which the amount of our belief of one proposition varies with the amount of our belief of other propositions with which it is connected.

---

[1] In the ordinary signification of this term. As De Morgan uses it he makes Formal Logic *include* Probability, as one of its branches, as indicated in his title "Formal Logic, on the Calculus of Inference, necessary and probable."

§ 4. If this were the opinion of De Morgan only, or even of mathematicians generally (and I believe that substantially the same opinion is adopted by most of those who have treated the subject mathematically), it might be objected that their peculiar studies had given them a bias towards discovering the distinctions and accuracy of numbers in matters into which these qualities are not commonly supposed to enter. But it must be observed that a professed logician, Archbishop Thomson, has to a considerable extent adopted the same opinion. In his work on the Laws of Thought he gives a separate section to the treatment of what he calls 'Syllogisms of Chance.' He prefaces it with a statement that the substance of the section is extracted from the works of De Morgan, and others who agree with De Morgan; he also makes a quotation from Donkin, with which he seems to agree, which declares that the subject matter of the science of Probability is 'quantity of belief'. This portion of the Archbishop's work cannot, it must be admitted, be regarded as very accurate or acute; it is referred to here only in order to show that the opinion now under discussion is by no means confined to mathematicians, but has been recognized and adopted by men who certainly cannot be charged with being subject to a mathematical bias.

---

[1] *Formal Logic.* Preface, page v.

§ 5. Before proceeding to criticise this opinion, one remark may be made upon it which has been too frequently overlooked. It should be borne in mind that, even were this view of the subject not actually incorrect, it might be objected to as insufficient for the purpose of a definition, on the ground that variation of belief is not confined to Probability. It is a property with which that science is concerned, no doubt, but it is a property which meets us in other directions as well. In every case in which we extend our inferences by Induction or Analogy, or depend upon the witness of others, or trust to our own memory of the past, or come to a conclusion through conflicting arguments, or even make a long and complicated deduction by mathematics or logic, we have a result of which we can scarcely feel as certain as of the premises from which it was obtained. In all these cases then we are conscious of varying quantities of belief, but are the laws according to which the belief is produced and varied the same? If they cannot be reduced to one harmonious scheme, if in fact they can at best be brought to nothing but a number of different schemes, each with its own body of laws and rules, then it is vain to endeavour to force them into one science.

This opinion is strengthened by observing that most of the writers who adopt the definition in question do practically dismiss from consideration most of the above-mentioned examples of diminution of belief, and confine their attention to classes of events which have the property discussed in Chap. I., viz. 'ignorance of the few, knowledge of the many.' It is quite true that considerable violence has to be done to some of these examples, by introducing exceedingly arbitrary suppositions into them, before they can be forced to assume a suitable form. But still there is little doubt that, if we carefully examine the language employed, we shall find that in almost every case assumptions are made which virtually imply that our knowledge of the individual is derived from propositions given in the typical form described in Chap. I. This will be more fully proved when we come to consider some common misapplications of the science.

§ 6. Even then, if the above-mentioned view of the subject were correct, it would yet, I consider, be insufficient for the purpose of a definition; but it is at least very doubtful whether it is correct. Before we could properly assign to the belief side of the question the prominence given to it by De Morgan and others, certainly before the science could be defined from that side, it would be necessary, it appears, to establish the two following positions, against both of which strong objections can be brought.

(1) That our belief of every proposition is a thing which we can, strictly speaking, be said to measure; that there must be a certain amount of it in every case, which we can realize somehow in consciousness and refer to some standard so as to pronounce upon its value.

(2) That the value thus apprehended is the correct one according to the theory, viz. that it is the exact fraction of full conviction that it should be. This statement will perhaps seem somewhat obscure at first; it will be explained presently.

§ 7. (I) Now, in the first place, as regards the difficulty of obtaining any measure of the amount of our belief. One source of this difficulty is too obvious to have escaped notice; this is the disturbing influence produced on the quantity of belief by any strong emotion or passion. A deep interest in the matter at stake, whether it excite hope or fear, plays great havoc with the belief-meter, so that we must assume the mind to be quite unimpassioned in weighing the evidence. This is noticed and acknowledged by Laplace and others; but these writers seem to me to assume it to be the only source of error, and also to be of comparative unimportance. Even if it were the only source of error I cannot see that it would be unimportant. We experience hope or fear in so very many instances, that to omit such influences from consideration would be almost equivalent to saying that whilst we profess to consider the whole quantity of our belief we will in reality consider only a portion of it. Very strong feelings are, of course, exceptional, but we should nevertheless find that the emotional element, in some form or other, makes itself felt on almost every occasion. It is very seldom that we cannot speak of our surprise or expectation in reference to any particular event. Both of these expressions, but especially the former, seem to point to something more than mere belief. It is true that the word 'expectation' is generally defined in treatises on Probability as equivalent to belief; but it seems doubtful whether any one who attends to the popular use of the terms would admit that they were exactly synonymous. Be this however as it may, the emotional element is present upon almost every occasion, and its disturbing influence therefore is constantly at work.

§ 8. Another cause, which co-operates with the former, is to be found in the extreme complexity and variety of the evidence on which our belief of any proposition depends. Hence it results that our actual belief at any given moment is one of the most fugitive and variable things possible, so that we can scarcely ever get sufficiently clear hold of it to measure it. This is not confined to the times when our minds are in a turmoil of excitement through hope or fear. In our calmest moments we shall find it no easy thing to give a precise answer to the question, How firmly do I hold this or that belief? There may be one or two prominent arguments in its favour, and one or two corresponding objections against it, but this is far from comprising all the causes by which our state of belief is produced. Because such reasons as these are all that can be practically introduced into oral or written controversies, we must not conclude that it is by these only that our conviction is influenced. On the contrary, our conviction generally rests upon a sort of chaotic basis composed of an infinite number of inferences and analogies of every description, and these moreover distorted by our state of feeling at the time, dimmed by the degree of our recollection of them afterwards, and probably received from time to time with varying force according to the way in which they happen to combine in our consciousness at the moment. To borrow a striking illustration from Abraham Tucker, the substructure of our convictions is not so much to be compared to the solid foundations of an ordinary building, as to the piles of the houses of Rotterdam which rest somehow in a deep bed of soft mud. They bear their weight securely enough, but it would not be easy to point out accurately the dependence of the different parts upon one another. Directly we begin to think of the amount of our belief, we have to think of the arguments by which it is produced—in fact, these arguments will intrude themselves without our choice. As each in turn flashes through the mind, it modifies the strength of our conviction; we are like a person listening to the confused hubbub of a crowd, where there is always something arbitrary in the particular sound we choose to listen to. There may be reasons enough to suffice abundantly for our ultimate choice, but on examination we shall find that they are by no means apprehended with the same force at different times. The belief produced by some strong argument may be very decisive at the moment, but it will often begin to diminish when the argument is not actually before the mind. It is like being dazzled by a strong light; the impression still remains, but begins almost immediately to fade away. I think that this is the case, however we try to limit the sources of our conviction.

§ 9. (II) But supposing that it were possible to strike a sort of average of this fluctuating state, should we find this average to be of the amount assigned by theory? In other words, is our natural belief in the happening of two different events in direct proportion to the frequency with which those events happen in the long run? There is a lottery with 100 tickets and ten prizes; is a man's belief that he will get a prize fairly represented by one-tenth of certainty? The mere reference to a lottery should be sufficient to disprove this. Lotteries have flourished at all times, and have never failed to be abundantly supported, in spite of the most perfect conviction, on the part of many, if not most, of those who put into them, that in the long run all will lose. Deductions should undoubtedly be made for those who act from superstitious motives, from belief in omens, dreams, and so on. But apart from these, and supposing any one to come fortified by all that mathematics can do for him, it is difficult to believe that his natural impressions about single events would be always what they should be according to theory. Are there many who can honestly declare that they would have no desire to buy a single ticket? They would probably say to themselves that the sum they paid away was nothing worth mentioning to lose, and that there was a chance of gaining a great deal; in other words, they are not apportioning their belief in the way that theory assigns.

What bears out this view is, that the same persons who would act in this way in single instances, would often not think of doing so in any but single instances. In other words, the natural tendency here is to attribute too great an amount of belief where it is or should be small; i.e. to depreciate the risk in proportion to the contingent advantage. They would very likely, when argued with, attach disparaging epithets to this state of feeling, by calling it an unaccountable fascination, or something of that kind, but of its existence there can be little doubt. We are speaking now of what is the natural tendency of our minds, not of that into which they may at length be disciplined by education and thought. If, however, educated persons have succeeded for the most part in controlling this tendency in games of chance, the 'spirit of reckless speculation' has scarcely yet been banished from commerce. On examination, this tendency will be found so prevalent in all ages, ranks, and dispositions, that it would be inadmissible to neglect it in order to bring our supposed instincts more closely into accordance with the commonly received theories of Probability.

§ 10. There is another aspect of this question which has been often overlooked, but which seems to deserve some attention. Granted that we have an instinct of credence, why should it be assumed that it must be just of that intensity which subsequent experience will justify? Our instincts are implanted in us for good purposes, and are intended to act immediately and unconsciously. They are, however, subject to control, and have to be brought into accordance with what we believe to be true and right. In other departments of psychology we do not assume that every spontaneous prompting of nature is to be left just as we find it, or even that on the average, omitting individual variations, it is set at that pitch that will be found in the end to be the best when we come to think about it and assign it its rules. Take, for example the case of resentment. Here we have an instinctive tendency, and one that on the whole is good in its results. But moralists are agreed that almost all our efforts at self-control are to be directed towards subduing it and keeping it in its right direction. It is assumed to be given as a sort of rough protection, and to be set, if one might so express oneself, at too high a pitch to be deliberately and consciously acted on in society. May not something of this kind be the case also with our belief? I only make a passing reference to this point here, as on the theory of Probability adopted in this work it does not seem to be at all material to the science. But it seems a strong argument against the expediency of commencing the study of the science from the subjective side, or even of assigning any great degree of prominence to this side.

That men *do* not believe in exact accordance with this theory must have struck almost every one, but this has probably been considered as mere exception and irregularity; the assumption being made that on the average, and in far the majority of cases, they do so believe. As stated above, it is very doubtful whether the tendency which has just been discussed is not so widely prevalent that it might with far more propriety be called the rule than the exception. And it may be better that it should be so: many good results may follow from that cheerful disposition which induces a man sometimes to go on trying after some great good, the chance of which he overvalues. He will keep on through trouble and disappointment, without serious harm perhaps, when the cool and calculating bystander sees plainly that his 'measure of belief' is much higher than it should be. So, too, the tendency also so common, of underrating the chance of a great evil may also work for good. By many *men* death might be looked upon as an almost infinite evil, at least they would so regard it themselves; suppose they kept this contingency constantly before them at its right value, how would it be possible to get through the practical work of life? Men would be stopping indoors because if they went out they might be murdered or bitten by a mad dog. To say this is not to advocate a return to our instincts; indeed when we have once reached the critical and conscious state, it is hardly possible to do so; but it should be noticed that the advantage gained by correcting them is at best but a balanced one[1]. What is most to our present purpose, it suggests the inexpediency of attempting to found an exact theory on what may afterwards prove to be a mere instinct, unauthorized in its full extent by experience.

§ 11. It may be replied, that though people, as a matter of fact, do not apportion belief in this exact way, yet they *ought* to do so. The purport of this remark will be examined presently; it need only be said here that it grants all that is now contended for. For it admits that the degree of our belief is capable of modification, and may need it. But in accordance with what is the belief to be modified? obviously in accordance with experience; it cannot be trusted to by itself, but the fraction at which it is to be rated must be determined by the comparative frequency of the events to which it refers. Experience then furnishing the standard, it is surely most reasonable to start from this experience, and to found the theory of our process upon it.

---

[1] An illustration of the points above insisted on has recently been given in a quarter where few would have expected it; I allude, as many readers will readily infer, to J. S. Mill's exceedingly interesting Essays on Theism. It is not within our province here to criticise any of their conclusions, but they have expressed in a very significant way the conviction entertained by him that beliefs which are not justified by evidence, and possibly may not be capable of justification, (those for instance of immortality and the existence of the Deity), may nevertheless not only continue to exist in cultivated minds, but may also be profitably encouraged there, at any rate in the shape of hopes, for certain supposed advantages attendant on their retention, irrespective even of their truth.

If we do not do this, it should be observed that we are detaching Probability altogether from the study of things external to us, and making it nothing else in effect than a portion of Psychology. If we refuse to be controlled by experience, but confine our attention to the laws according to which belief is naturally or instinctively compounded and distributed in our minds, we have no right then to appeal to experience afterwards even for illustrations, unless under the express understanding that we do not guarantee its accuracy. Our belief in some single events, for example, might be correct, and yet that in a compound of several (if derived merely from our instinctive laws of belief) very possibly might not be correct, but might lead us into practical mistakes if we determined to act upon it. Even if the two were in accordance, this accordance would have to be proved, which would lead us round, by what I cannot but think a circuitous process, to the point which has been already chosen for commencing with.

§ 12. De Morgan seems to imply that the doctrine criticised above finds a justification from the analogy of Formal Logic. If the laws of necessary inference can be studied apart from all reference to external facts (except by way of illustration), why not those of probable inference? There does not, however, seem to be much force in any such analogy. Formal Logic, at any rate under its modern or Kantian mode of treatment, is based upon the assumption that there are laws of thought as distinguished from laws of things, and that these laws of thought can be ascertained and studied without taking into account their reference to any *particular* object. Now so long as we are confined to necessary or irreversible laws, as is of course the case in ordinary Formal Logic, this assumption leads to no special difficulties. We mean by this, that no conflict arises between these subjective and objective necessities. The two exist in perfect harmony side by side, the one being the accurate counterpart of the other. So precise is the correspondence between them, that few persons would notice, until study of metaphysics had called their attention to such points, that there were these two sides to the question. They would make their appeal to either with equal confidence, saying indifferently, 'the thing must be so,' or, 'we cannot conceive its being otherwise.' In fact it is only since the time of Kant that this mental analysis has been to any extent appreciated and accepted. And even now the dominant experience school of philosophy would not admit that there are here two really distinct sides to the phenomenon; they maintain either that the subjective necessity is nothing more than the consequence by inveterate association of the objective uniformity, or else that this so-called necessity (say in the Law of Contradiction) is after all merely verbal, merely a different way of saying the same thing over in other words. Whatever the explanation adopted, the general result is that fallacies, as real acts of thought, are impossible within the domain of pure logic; error within that province is only possible by a momentary lapse of attention, that is of consciousness.

§ 13. But though this perfect harmony between the subjective and the objective uniformities or laws exist within the domain of pure logic, it is far from existing within that of probability. The moment we make the *quantity* of our belief an integral part of the subject to be studied, any such invariable correspondence ceases to exist. In the former case, we could not consciously think erroneously even though we might try to do so; in the latter, we not only can believe erroneously but constantly do so. Far from the quantity of our belief being so exactly adjusted in conformity with the facts to which it refers that we cannot even in imagination go astray, we find that it frequently exists in excess or defect of that which subsequent judgment will approve of. Our instincts of credence are unquestionably in frequent hostility with experience; and what do we do then? We simply modify the instincts into accordance with the things. We are constantly performing this practice, and no cultivated mind would find it possible to do anything else. No man would think of divorcing his belief from the things on which it was exercised, or would suppose that the former had anything else to do than to follow the lead of the latter. Hence it results that that separation of the subjective necessity from the objective, and that determination to treat the former as a science apart by itself, for which a plausible defence could be made in the case of pure logic, is entirely inadmissible in the case of probability. However we might contrive to '*think*' aright without appeal to facts, we cannot *believe* aright without incessantly checking our proceedings by such appeals. Whatever then may be the claims of Formal Logic to rank as a separate science, it does not appear that it can furnish any support to the theory of Probability at present under examination.

§ 14. The point in question is sometimes urged as follows. Suppose a man with two, and only two, alternatives before him, one of which he knows must involve success and the other failure. He knows nothing more about them than this, and he is forced to act. Would he not regard them with absolutely similar and equal feelings of confidence, without the necessity of referring them to any real or imaginary series? If so, is not this equivalent to saying that his belief of either, since one of them must come to pass, is equal to that of the other, and therefore that his belief of each is one-half of full confidence? Similarly if there are more than two alternatives: let it be supposed that there are any number of them, amongst which no distinctions whatever can be discerned except in such particulars as we know for certain will not affect the result; should we not feel equally confident in respect of each of them? and so here again should we not have a fractional estimate of our absolute amount of belief? It is thus attempted to lay the basis of a pure science of Probability, determining the distribution and combination of our belief hypothetically; viz. *if* the contingencies are exactly alike, then our belief is so apportioned, the question whether the contingencies are equal being of course decided as the objective data of Logic or Mathematics are decided.

To discuss this question fully would require a statement at some length of the reasons in favour of the objective or material view of Logic, as opposed to the Formal or Conceptualist. I shall have to speak on this subject in another chapter, and will not therefore enter upon it here. But one conclusive objection which is applicable more peculiarly to Probability may be offered at once. To pursue the line of enquiry just indicated, is, as already remarked, to desert the strictly logical ground, and to take up that appropriate to psychology; the proper question, in all these cases, being not what *do* men believe, but what ought they to believe? Admitting, as was done above, that in the case of Formal Logic these two enquiries, or rather those corresponding to them, practically run into one, owing to the fact that men cannot consciously 'think' wrongly; it cannot be too strongly insisted on that in Probability the two are perfectly separable and distinct. It is of no use saying what men do or will believe, we want to know what they will be right in believing; and this can never be settled without an appeal to the phenomena themselves.

§ 15. But apart from the above considerations, this way of putting the case does not seem to me at all conclusive. Take the following example. A man[1] finds himself on the sands of the Wash or Morecambe Bay in a dense mist, when the spring-tide is coming in; and knows therefore that to be once caught by the tide would be fatal. He hears a church-bell at a distance, but has no means of knowing whether it is on the same side of the water with himself or on the opposite side. He cannot tell therefore whether by following its sound he will be led out into the mid-stream and be lost, or led back to dry land and safety. Here there can be no repetition of the event, and the cases are indistinguishably alike, to him, in the only circumstances which can affect the issue: is not then his prospect of death, it will be said, necessarily equal to one half? A proper analysis of his state of mind would be a psychological rather than a logical enquiry, and in any case, as above remarked, the decision of this question does not touch our logical position. But according to the best introspection I can give I should say that what really passes through the mind in such a case is something of this kind: In all doubtful positions and circumstances we are accustomed to decide our conduct by a consideration of the relative advantages and disadvantages of each side, that is by the observed or inferred frequency with which one or the other alternative has succeeded. In proportion as these become more nearly balanced, we are more frequently mistaken in the individual cases; that is, it becomes more and more nearly what would be called 'a mere toss up' whether we are right or wrong. The case in question seems merely the limiting case, in which it has been contrived that there shall be no appreciable difference between the alternatives, by which to decide in favour of one or other, and we accordingly feel no confidence in the particular result. Having to decide, however, we decide according to the precedent of similar cases which have occurred before. To stand still and wait for better information is certain death, and we therefore appeal to and employ the only rule we know of; or rather we feel, or endeavour to feel, as we have felt before when acting in the presence of alternatives as nearly balanced as possible. But I can neither perceive in my own case, nor feel convinced in that of others, that this appeal, in a case which cannot be repeated[1], to a rule acted on and justified in cases which can be and are repeated, at all forces us to admit that our state of mind is the same in each case.

---

[1] It is necessary to take an example in which the man is forced to act, or we should not be able to shew that he has any belief on the subject at all. He may declare that he neither knows nor cares anything about the matter, and that therefore there is nothing of the nature of belief to be extracted out of his mental condition. He very likely would take this ground if we asked him, as De Morgan does, with a slightly different reference (*Formal Logic*, p. 183), whether he considers that there are volcanoes on the unseen side of the moon larger than those on the side turned towards us; or, with Mr Jevons (*Principles of Science*, Vol. I. p. 243) whether he considers that a Platythliptic Coefficient is positive. These do not therefore seem good instances to illustrate the position that we always entertain a certain degree of belief on every question which can be stated, and that utter inability to give a reason in favour of either alternative corresponds to half belief.

§ 16. This example serves to bring out very clearly a point which has been already mentioned, and which will have to be insisted upon again, viz. that all which Probability discusses is the statistical frequency of events, or, if we prefer so to put it, the quantity of belief with which any one of these events should be individually regarded, but leaves all the subsequent conduct dependent upon that frequency, or that belief, to the choice of the agents. Suppose there are two travellers in the predicament in question: shall they keep together, or separate in opposite directions? In either case alike the chance of safety to each is the same, viz. one-half, but clearly their circumstances must decide which course it is preferable to adopt. If they are husband and wife, they will probably prefer to remain together; if they are sole depositaries of an important state secret, they will decide to part. In other words, we have to select here between the two alternatives of the certainty of a single loss, and the even chance of a double loss; alternatives which the common mathematical statement of their chances has a decided tendency to make us regard as indistinguishable from one another. But clearly the decision must be grounded on the desires, feelings, and conscience of the agents. Probability cannot say a word upon this question. As I have pointed out elsewhere, there has been much confusion on this matter in applications of the science to betting, and in the discussion of the Petersburg problem.

---

[1] Except indeed on the principles indicated further on in §§ 24, 25.

We have thus examined the doctrine in question with a minuteness which may seem tedious, but in consequence of the eminence of its supporters it would have been presumptuous to have rejected it without the strongest grounds. The objections which have been urged might be summarised as follows:—the amount of our belief of any given proposition, supposing it to be in its nature capable of accurate determination (which does not seem to be the case), depends upon a great variety of causes, of which statistical frequency—the subject of Probability—is but one. That even if we confine our attention to this one cause, the natural amount of our belief is not necessarily what theory would assign, but has to be checked by appeal to experience. The subjective side of

Probability therefore, though very interesting and well deserving of examination, seems a mere appendage of the objective, and affords in itself no safe ground for a science of inference.

§ 17. The conception then of the science of Probability as a science of the laws of belief seems to break down at every point. We must not however rest content with such merely negative criticism. The degree of belief we entertain of a proposition may be hard to get at accurately, and when obtained may be often wrong, and may need therefore to be checked by an appeal to the objects of belief. Still in popular estimation we do seem to be able with more or less accuracy to form a graduated scale of intensity of belief. What we have to examine now is whether this be possible, and, if so, what is the explanation of the fact?

That it is generally believed that we can form such a scale scarcely admits of doubt. There is a whole vocabulary of common expressions such as, 'I feel almost sure,' 'I do not feel quite certain,' 'I am less confident of this than of that,' and so on. When we make use of any one of these phrases we seldom doubt that we have a distinct meaning to convey by means of it. Nor do we feel much at a loss, under any given circumstances, as to which of these expressions we should employ in preference to the others. If we were asked to arrange in order, according to the intensity of the belief with which we respectively hold them, things broadly marked off from one another, we could do it from our consciousness of belief alone, without a fresh appeal to the evidence upon which the belief depended. Passing over the looser propositions which are used in common conversation, let us take but one simple example from amongst those which furnish numerical data. Do I not feel more certain that some one will die this week in the whole town, than in the particular street in which I live? and if the town is known to contain a population one hundred times greater than that in the street, would not almost any one be prepared to assert on reflection that he felt a hundred times more sure of the first proposition than of the second? Or to take a non-numerical example, are we not often able to say unhesitatingly which of two propositions we believe the most, and to some rough degree how much more we believe one than the other, at a time when all the evidence upon which each rests has faded from the mind, so that each has to be judged, as we may say, solely on its own merits?

Here then a problem proposes itself. If popular opinion, as illustrated in common language, be correct,—and very considerable weight must of course be attributed to it,—there does exist something which we call partial belief in reference to any proposition of the numerical kind described above. Now what we want to do is to find some test or justification of this belief, to obtain in fact some intelligible answer to the question, Is it correct? We shall find incidentally that the answer to this question will throw a good deal of light upon another question nearly as important and far more intricate, viz. What is the meaning of this partial belief?

§ 18. We shall find it advisable to commence by ascertaining how such enquiries as the above would be answered in the case of ordinary full belief. Such a step would not offer the slightest difficulty. Suppose, to take a simple example, that we have obtained the following proposition,—whether by induction, or by the rules of ordinary deductive logic, does not matter for our present purpose,—that a certain mixture of oxygen and hydrogen is explosive. Here we have an inference, and consequent belief of a proposition. Now *suppose* there were any enquiry as to whether our belief were correct, what should we do? The simplest way of settling the matter would be to find out by a distinct appeal to experience whether the proposition was true. Since we are reasoning about things, the justification of the belief, that is, the test of its correctness, would be most readily found in the truth of the proposition. If by any process of inference I have come to believe that a certain mixture will explode, I consider my belief to be justified, that is to be correct, if under proper circumstances the explosion always does occur; if it does not occur the belief was wrong.

Such an answer, no doubt, goes but a little way, or rather no way at all, towards explaining what is the nature of belief in itself; but it is sufficient for our present purpose, which is merely that of determining what is meant by the correctness of our belief, and by the test of its correctness. In all inferences about things, in which the *amount* of our belief is not taken into account, such an explanation as the above is quite sufficient; it would be the ordinary one in any question of science. It is moreover perfectly intelligible, whether the conclusion is particular or universal. Whether we believe that 'some men die', or that 'all men die', our belief may with equal ease be justified by the appropriate train of experience.

§ 19. But when we attempt to apply the same test to *partial* belief, we shall find ourselves reduced to an awkward perplexity. A difficulty now emerges which has been singularly overlooked by those who have treated of the subject. As a simple example will serve our purpose, we will take the case of a penny. I am about to toss one up, and I therefore half believe, to adopt the current language, that it will give head. Now it seems to be overlooked that if we appeal to the event, as we did in the case last examined, our belief must inevitably be wrong, and therefore the test above mentioned will fail. For the thing must either happen or not happen: i.e. in this case the penny must either give head, or not give it; there is no third alternative. But whichever way it occurs, our half-belief, so far as such a state of mind admits of interpretation, must be wrong. If head does come, I am wrong in not having expected it enough; for I only half believed in its occurrence. If it does not happen, I am equally wrong in having expected it too much; for I half believed in its occurrence, when in fact it did not occur at all.

The same difficulty will occur in every case in which we attempt to justify our state of partial belief in a single contingent event. Let us take another example, slightly differing from the last. A man is to receive £1 if a die gives six, to pay 1*s.* if it gives any other number. It will generally be admitted that he ought to give 2*s.* 6*d.* for the chance, and that if he does so he will be paying a fair sum. This example only differs from the last in the fact that instead of simple belief in a proposition, we have taken what mathematicians call 'the *value* of the expectation'. In other words, we have brought into greater prominence, not merely the belief, but the conduct which is founded upon the belief. But precisely the same difficulty recurs here. For appealing to the event,—the single event, that is,—we see that one or other party must lose his money without compensation. In what sense then can such an expectation be said to be a fair one?

§ 20. A possible answer to this, and so far as appears the only possible answer, will be, that what we really mean by saying that we half believe in the occurrence of head is to express our conviction that head will certainly happen on the average every other time. And similarly, in the second example, by calling the sum a fair one it is meant that in the long run neither party will gain or lose. As we shall recur presently to the point raised in this form of answer, the *only* notice that need be taken of it at this point is to call attention to the fact that it entirely abandons the whole question in dispute, for it admits that this partial belief does not in any strict sense apply to the individual event, since it clearly cannot be justified there. At such a result indeed we cannot be surprised; at least we cannot on the theory adopted throughout this Essay. For bearing in mind that the employment of Probability postulates ignorance of the single event, it is not easy to see how we are to justify any other opinion or statement about the single event than a confession of such ignorance.

§ 21. So far then we do not seem to have made the slightest approximation to a solution of the particular question now under examination. The more closely we have analysed special examples, the more unmistakeably are we brought to the conclusion that in the individual instance no justification of anything like quantitative belief is to be found; at least none is to be found in the same sense in which we expect it in ordinary scientific conclusions, whether Inductive or Deductive. And yet we have to face and account for the fact that common impressions, as attested by a whole vocabulary of common phrases, are in favour of the existence of this quantitative belief. How are we to account for this? If we appeal to an example again, and analyse it somewhat more closely, we may yet find our way to some satisfactory explanation.

In our previous analysis (§ 18) we found it sufficient to stop at an early stage, and to give as the justification of our belief the fact of the proposition being true. Stopping however at that stage, we have found this explanation fail altogether to give a justification of partial belief; fail, that is, when applied to the individual instance. The two states of belief and disbelief correspond admirably to the two results of the event happening and not happening respectively, and unless for psychological purposes we saw no reason to analyse further; but to partial belief there is nothing corresponding in the result, for the event cannot partially happen in such cases as we are concerned with. Suppose then we advance a step further in the analysis, and ask again what is meant by the proposition being true? This introduces us, of course, to a very long and intricate path; but in the short distance along it which we shall advance, we shall not, it is to be hoped, find any very serious difficulty. As before, we will illustrate the analysis by first applying it to the case of ordinary full belief.

§ 22. Whatever opinion then may be held about the essential nature of belief, it will probably be admitted that a readiness to act upon the proposition believed is an inseparable accompaniment of that state of mind. There can be no alteration in our belief (at any rate in the case of sane persons) without a possible alteration in our conduct, nor anything in our conduct which is not connected with something in our belief. We will first take an example in connection with the penny, in which there is full belief; we will analyse it a step further than we did before, and then attempt to apply the same analysis to an example of a similar kind, but one in which the belief is partial instead of full.

Suppose that I am about to throw a penny up, and contemplate the prospect of its falling upon one of its sides and not upon its edge. We feel perfectly confident that it will do so. Now whatever else may be implied in our belief, we certainly mean this; that we are ready to stake our conduct upon its falling thus. All our betting, and everything else that we do, is carried on upon this supposition. Any risk whatever that might ensue upon its falling otherwise will be incurred without fear. This, it must be observed, is equally the case whether we are speaking of a single throw or of a *long* succession of throws.

But now let us take the case of a penny falling, not upon one side or the other, but upon a given side, *head.* To a certain extent this example resembles the last. We are perfectly ready to stake our conduct upon what comes to pass in the long run. When we are considering the result of a large number of throws, we are ready to act upon the supposition that head comes every other time. If e.g. we are betting upon it, we shall not object to paying £1 every time that head comes, on condition of receiving £1 every time that head does not come. This is nothing else than the *translation,* as we may call it, into practice, of our belief that head and tail occur equally often.

Now it will be obvious, on a moment's consideration, that our conduct is capable of being slightly varied, of being varied, that is, in form, whilst it remains identical in respect of its results. It is clear that to pay £1 every time we lose, and to get £1 every time we gain, comes to precisely the same thing, in the case under consideration, as to pay ten shillings every time without exception, and to receive £1 every time that head occurs. It is so, because heads occur, on the average, every other time. In the long run the two results coincide; but there is a marked difference between the two cases, considered individually. The difference is two-fold. In the first place we have slid from the notion of a payment every other time, and come to that of one made every time. In the second place, what we pay every time is half of what we get in the cases in which we do get anything. The difference may seem slight; but mark the effect when our conduct is translated back again into the subjective condition upon which it depends, viz. into our belief. It is in consequence of such a translation, as it appears to me, that the notion has been acquired that we have an accurately determinable amount of belief as to every such proposition. To have losses and gains of equal amount, and to incur them equally often, was the experience connected with our belief that the two events, head and tail, would occur equally often. This was quite intelligible, for it referred to the long run. To find that this could be commuted for a payment made every time without exception, a payment, observe, of half the amount of what we occasionally receive, has very naturally been interpreted to mean that there must be a state of half-belief which refers to each individual throw.

§ 23. One such example, of course, does not go far towards establishing a theory. But the reader will bear in mind that almost all our conduct tends towards the same result; that it is not in betting only, but in every course of action in which we have to count the events, that such a numerical apportionment of our conduct is possible. Hence, by the ordinary principles of association, it would appear exceedingly likely that, not exactly a numerical condition of mind, but rather numerical associations, become inseparably connected with each particular event which we know to occur in a certain proportion of times. Once in six times a die gives ace; a knowledge of this fact, taken in combination with all the practical results to which it leads, produces, one cannot doubt, an inseparable notion of one-sixth connected with each *single* throw. But it surely cannot be called belief to the amount of one-sixth; at least it admits neither of justification nor explanation in these single cases, to which alone the fractional belief, if such existed, ought to apply.

It is in consequence, I apprehend, of such association that we act in such an unhesitating manner in reference to any single contingent event, even when we have no expectation of its being repeated. A die is going to be thrown up once, and once only. I bet 5 to 1 against ace, not, as is commonly *asserted,* because I feel one-sixth part of certainty in the occurrence of ace; but because I know that such conduct would be justified in the long run of such cases, and I apply to the solitary individual the same rule that I should apply to it if I knew it were one of a long series. This accounts for my conduct being the same in the two cases; by association, moreover, we probably experience very similar feelings in regard to them both.

§ 24. And here, on the view of the subject adopted in this Essay, we might stop. We are bound to explain the 'measure of our belief' in the occurrence of a single event when we judge solely from the statistical frequency with which such events occur, for such a series of events was our starting-point; but we are not bound to inquire whether in every case in which persons have, or claim to have, a certain measure of belief there must be such a series to which to refer it, and by which to justify it. Those who start from the subjective side, and regard Probability as the science of quantitative belief, are obliged to do this, but we are free from the obligation.

Still the question is one which is so naturally raised in connection with this subject, that it cannot be altogether passed by. I think that to a considerable extent such a justification as that mentioned above will be found applicable in other cases. The fact is that we are very seldom called upon to decide and act upon a single contingency which cannot be viewed as being one of a series. Experience introduces us, it must be remembered, not merely to a succession of events neatly arranged in a single series (as we have hitherto assumed them to be for the purpose of illustration), but to an infinite number belonging to a vast variety of different series. A man is obliged to be acting, and therefore exercising his belief about one thing or another, almost the whole of every day of his life. Any one person will have to decide in his time about a multitude of events, each one of which may never recur again within his own experience. But by the very fact of there being a multitude, though they are all of different kinds, we shall still find that order is maintained, and so a course of conduct can be justified. In a plantation of trees we should find that there is order of a certain kind if we measure them in any one direction, the trees being on an average about the same distance from each other. But a somewhat similar order would be found if we were to examine them in any other direction whatsoever. So in nature generally; there is regularity in a succession of events of the same kind. But there may also be regularity if we form a series by taking successively a number out of totally distinct kinds.

It is in this circumstance that we find an extension of the practical justification of the measure of our belief. A man, say, buys a life annuity, insures his life on a railway journey, puts into a lottery, and so on. Now we may make a series out of these acts of his, though each is in itself a single event which he may never intend to repeat. His conduct, and therefore his belief, measured by the result in each individual instance, will not be justified, but the reverse, as shewn in § 19. Could he indeed repeat each kind of action often enough it would be justified; but from this, by the conditions of life, he is debarred. Now it is perfectly conceivable that in the new series, formed by his successive acts of different kinds, there should be no regularity. As a matter of fact, however, it is found that there is regularity. In this way the equalization of his gains and losses, for which he cannot hope in annuities, insurances, and lotteries taken separately, may yet be secured to him out of these events taken collectively. If in each case he values his chance at its right proportion (and acts accordingly) he will in the course of his life neither gain nor lose. And in the same way if, whenever he has the alternative of different courses of conduct, he acts in accordance with the estimate of his belief described above, i.e. chooses the event whose chance is the best, he will in the end gain more in this way than by any other course. By the existence, therefore, of these *cross-series*, as we may term them, there is an immense addition to the number of actions which may be fairly considered to belong to those courses of conduct which offer many successive opportunities of equalizing gains and losses. All these cases then may be regarded as admitting of justification in the way now under discussion.

§ 25. In the above remarks it will be observed that we have been giving what is to be regarded as a justification of his belief from the point of view of the individual agent himself. If we suppose the existence of an enlarged fellow-feeling, the applicability of such a justification becomes still more extensive. We can assign a very intelligible sense to the assertion that it is 999 to 1 that I shall not get a prize in a lottery, even if this be stated in the form that my belief in my so doing is represented by the fraction $\frac{1}{1000}$th of certainty. Properly it means that in a very large number of throws I should gain once in 1000 times. If we admit other contingencies of the same kind, as described in the last section, each individual may be supposed to reach to something like this experience within the limits of his own life. He could not do it in this particular line of conduct alone, but he could do it in this line combined with others. Now introduce the possibility of each man feeling that the gain of others offers some analogy to his own gains, which we may conceive his doing except in the case of the gains of those against whom he is directly competing, and the above justification becomes still more extensively applicable.

The following would be a fair illustration to test this view. I know that I must die on some day of the week, and there are but seven days. My belief, therefore, that I shall die on a Sunday is one-seventh. Here the contingent event is clearly one that does not admit of repetition; and yet would not the belief of every man have the value assigned it by the formula? It would appear that the same principle will be found to be at work here as in the former examples. It is quite true that I have only the opportunity of dying once myself, but I am a member of a class in which deaths occur with frequency, and I form my opinion upon evidence drawn from that class. If, for example, I had insured my life for £1000, I should feel a certain propriety in demanding £7000 in case the office declared that it would only pay in the event of my dying on a Sunday. *I*, or the office, as the case might be, might not find the arrangement an equitable one, but mankind at large, in case they acted on such a principle, might fairly commute their aggregate gains in such a way, whilst to the Insurance Office it would not make any difference at all.

§ 26. The results of the last few sections might be summarised as follows:—the different amounts of belief which we entertain upon different events, and which are recognized by various phrases in common use, have undoubtedly some meaning. But the greater part of their meaning, and certainly their only justification, are to be sought in the *series* of corresponding events to which they belong; in regard to which it may be shewn that far more events are capable of being referred to a series than might be supposed at first sight. The test and justification of belief are to be found in conduct; in this test applied to the series as a whole, there is nothing peculiar, it differs in no way from the similar test when we are acting on our belief about any single event. But so applied, from the nature of the case it is applied successively to each of the individuals of the series; here our *conduct* generally admits of being separately considered in reference to each particular event; and this has been understood to denote a certain amount of belief which should be a fraction of certainty. Probably on the principles of association, a peculiar condition of mind is produced in reference to each single event. And these associations are not unnaturally retained even when we contemplate any one of these single events isolated from any series to which it belongs. When it is found alone we treat it, and feel towards it, as we do when it is in company with the rest of the series.

§ 27. We may now see, more clearly than we could before, why it is that we are free from any necessity of assuming the existence of causation, in the sense of necessary invariable sequence, in the case of the events which compose our series. Against such a view it might very plausibly be urged, that we constantly talk of the probability of a single event; but how can this be done, it may reasonably be said, if we once admit the possibility of that event occurring fortuitously? Take an instance from human life; the average duration of the lives of any batch of men aged thirty will be about thirty-four years. We say therefore to any individual of them, Your expectation of life is thirty-four years. But how can this be said if we admit that the train of events composing his life is liable to be destitute of all regular sequence of cause and effect? To this it may be replied that the denial of causation enables us to say neither more nor less than its assertion, in reference to the length of the individual life, for of this we are ignorant in each case alike. By assigning, as above, an expectation in reference to the individual, we *mean* nothing more than to make a statement about the average of his class. Whether there be causation or not in these individual cases does not affect our knowledge of the average, for this by supposition rests on independent experience. The legitimate inferences are the same on either hypothesis, and of equal value. The only difference is that on the hypothesis of non-causation we have forced upon our attention the impropriety of talking of the 'proper' expectation of the individual, owing to the fact that all knowledge of its amount is formally impossible; on the other hypothesis the impropriety is overlooked from the fact of such knowledge being only practically unattainable. As a matter of fact the amount of our knowledge is the same in each case; it is a knowledge of the average, and of that only[1].

§ 28. We may conclude, then, that the limits within which we are thus able to justify the amount of our belief are far more extensive than might appear at first sight. Whether every case in which persons feel an amount of belief short of perfect confidence could be forced into the province of Probability is a wider question. Even, however, if the belief could be supposed capable of justification on its principles, its rules could never in such cases be made use of. Suppose, for example, that a father were in doubt whether to give a certain medicine to his sick child. On the one hand, the doctor declared that the child would die unless the medicine were given; on the other, through a mistake, the father cannot feel quite sure that the medicine he has is the right one. It is conceivable that some mathematicians, in their conviction that everything has its definite numerical probability, would declare that the man's belief had some 'value' (if they could only find out what it is), say nine-tenths; by which they would mean that in nine cases out of ten in which he entertained a belief of that particular value he proved to be right. So with his belief and doubt on the other side of the question. Putting the two together, there is but one course which, as a prudent man and a good father, he can possibly follow. It may be so, but when (as here) the identification of an event in a series depends on purely subjective conditions, as in this case upon the degree of vividness of his conviction, of which no one else can judge, no test is possible, and therefore no proof can be found.

---

[1] For a fuller discussion of this, see the Chapter on Causation.

§ 29. So much then for the attempts, so frequently made, to found the science on a subjective basis; they can lead, as it has here been endeavoured to show, to no satisfactory result. Still our belief is so inseparably connected with our action, that something of a defence can be made for the attempts described above; but when it is attempted, as is often the case, to import other sentiments besides pure belief, and to find a justification for them also in the results of our science, the confusion becomes far worse. The following extract from Archbishop Thomson's *Laws of Thought* (§ 122, Ed. II.) will show what kind of applications of the science are contemplated here: "In applying the doctrine of chances to that subject in connexion with which it was invented—games of chance,—the principles of what has been happily termed 'moral arithmetic' must not be forgotten. Not only would it be difficult for a gamester to find an antagonist on terms, as to fortune and needs, precisely equal, but also it is impossible that with such an equality the advantage of a considerable gain should balance the harm of a serious loss. 'If two men,' says Buffon, 'were to determine to play for their whole property, what would be the effect of this agreement? The one would only double his fortune, and the other reduce his to naught. What proportion is there between the loss and the gain? The same that there is between all and nothing. The gain of the one is but a moderate sum,—the loss of the other is numerically infinite, and morally so great that the labour of his whole life may not perhaps suffice to restore his property.'"

As moral advice this is all very true and good. But if it be regarded as a contribution to the science of the subject it is quite inappropriate, and seems calculated to cause confusion. The doctrine of chances pronounces upon certain kinds of events in respect of number and magnitude; it has absolutely nothing to do with any particular person's feelings about these relations. We might as well append a corollary to the rules of arithmetic, to point out that although it is very true that twice two are four it does not follow that four horses will give twice as much pleasure to the owner as two will. If two men play on equal terms their chances are equal; in other words, if they were often to play in this manner each would lose as frequently as he would gain. That is all that Probability can say; what under the circumstances may be the determination and opinions of the men in question, it is for them and them alone to decide. There are many persons who cannot bear mediocrity of any kind, and to whom the prospect of doubling their fortune would outweigh a greater chance of losing it altogether. They alone are the judges.

If we will introduce such a balance of pleasure and pain the individual must make the calculation for himself. The supposition is that total ruin is very painful, partial loss painful in a less proportion than that assigned by the ratio of the losses themselves; the inference is therefore drawn that on the average more pain is caused by occasional great losses than by frequent small ones, though the money value of the losses in the long run may be the same in each case. But if we suppose a country where the desire of spending largely is very strong, and where from abundant production loss is easily replaced, the calculation might incline the other way.

Under such circumstances it is quite possible that more happiness might result from playing for high than for low stakes. The fact is that all emotional considerations of this kind are irrelevant; they are, at most, mere applications of the theory, and such as each individual is alone competent to make for himself. Some more remarks will be made upon this subject in the chapter upon Insurance and Gambling.

§ 30. It is by the introduction of such considerations as these that the really very simple and intelligible Petersburg Problem has been so perplexed. Having already given some description of this problem we will refer to it very briefly here. It presents us with a sequence of sets of throws for each of which sets I am to receive something, say a shilling, as the minimum receipt. My receipts increase in proportion to the rarity of each particular kind of set, and each kind is observed or inferred to grow more rare in a certain definite but unlimited order. By the wording of the problem, properly interpreted, I am supposed never to stop. Clearly therefore, however large a fee I pay for each of these sets, I shall be sure to make it up in time. The mathematical expression of this is, that I ought always to pay an infinite sum. To this the objection is opposed, that no sensible man would think of advancing even a large finite sum, say £50. Certainly he would not; but why? Because neither he nor those who are to pay him would be likely to live long enough for him to obtain throws good enough to remunerate him for one-tenth of his outlay; to say nothing of his trouble and loss of time. We must not suppose that the problem, as stated in the ideal form, will coincide with the practical form in which it presents itself in life. A carpenter might as well object to Euclid's second postulate, because his plane came to a stop in six feet on the plank on which he was at work. Many persons have failed to perceive this, and have assumed that, besides enabling us to draw numerical inferences about the members of a series, the theory ought also to be called upon to justify all the opinions which average respectable men might be inclined to form about them, as well as the conduct they might choose to pursue in consequence. It is obvious that to enter upon such considerations as these is to diverge from our proper ground. We are concerned, in these cases, with the actions of men only, as given in statistics; with the emotions they experience in the performance of these actions we have no direct concern whatever. The error is the same as if any one were to confound, in political economy, value in use with value in exchange, and object to measuring the value of a loaf by its cost of production, because bread is worth more to a man when he is hungry than it is just after his dinner.

§ 31. One class of emotions indeed ought to be excepted, which, from the apparent uniformity and consistency with which they show themselves in different persons and at different times, do really present some better claim to consideration. In connection with a science of inference they can never indeed be regarded as more than an accident of what is essential to the subject, but compared with other emotions they seem to be inseparable accidents.

The reader will remember that attention was drawn in the earlier part of this chapter to the compound nature of the state of mind which we term belief. It is partly intellectual, partly also emotional; it professes to rest upon experience, but in reality the experience acts through the distorting media of hopes and fears and other disturbing agencies. So long as we confine our attention to the *state of mind* of the person who believes, it appears to me that these two parts of belief are quite inseparable. Indeed, to speak of them as two parts may convey a wrong impression;

for though they spring from different sources, they so entirely merge in one result as to produce what might be called an indistinguishable compound. Every kind of inference, whether in probability or not, is liable to be disturbed in this way. A timid man may honestly believe that he will be wounded in a coming battle, when others, with the same experience but calmer judgments, see that the chance is too small to deserve consideration. But such a man's belief, if we look only to that, will not differ in its nature from sound belief. His conduct also in consequence of his belief will by itself afford no ground of discrimination; he will make his will as sincerely as a man who is unmistakeably on his death-bed. The only resource is to check and correct his belief by appealing to past and current experience. This was advanced as an objection to the theory on which probability is regarded as concerned primarily with laws of belief. But on the view taken in this Essay in which we are supposed to be concerned with laws of inference about things, error and difficulty from this source vanish. Let us bear clearly in mind that we are concerned with inferences about things, and are always to test our belief by experience of the things, and whatever there may be in belief which does not depend on experience will disappear from notice. It is conceivable indeed that men might be in such a state of abject panic that their senses, at the time or afterwards, were disturbed like their judgment beforehand. If so, we must appeal to the wider experience of other men of calmer mind, or to their own judgment in their better moments. These are the ultimate, and apparently the only ultimate courts of appeal.

§ 32. These emotions then can claim no notice as an integral portion of any science of inference, and should in strictness be rigidly excluded from it. But if any of them are uniform and regular in their production and magnitude, they may be fairly admitted as accidental and extraneous accompaniments. This is really the case to some extent with our surprise. This emotion does show a considerable degree of uniformity. The rarer any event is the more am I, in common with most other men, surprised at it when it does happen. This surprise may range through all degrees, from the most languid form of interest up to the condition which we term being startled. And since the surprise seems to be pretty much the same, under similar circumstances, at different times, and in the case of different persons, it is free from that extreme irregularity which is found in most of the other mental conditions which accompany the contemplation of unexpected events. Hence our surprise, though, as stated above, having no proper claim to admission into the science of Probability, is such a constant and regular accompaniment of that which Probability is concerned with, that notice must often be taken of it. References will occasionally be found to this aspect of the question in the following chapters.

It may be remarked in passing, for the sake of further illustration of the subject, that this emotional accompaniment of surprise, to which we are thus able to assign something like a fractional value, differs in two important respects from the commonly accepted fraction of belief. In the first place, it has what may be termed an independent existence; it is intelligible by itself. The belief, as we endeavoured to show, needs explanation and finds it in our consequent conduct. Not so with the emotion; this stands upon its own footing, and may be examined in and by itself. Hence, in the second place, it is as applicable, and as capable of any kind of justification, in relation to the *single event*, as to a series of events. In this respect, as will be remembered, it offers a complete contrast to our state of belief about any one contingent event. May not these considerations help to account for the general acceptance of the doctrine, that we have a certain definite and measurable amount of belief about these events? I cannot help thinking that what is so obviously true of the emotional portion of the belief, has been unconsciously transferred to the other or intellectual portion of the compound condition, to which it is not applicable, and where it cannot find a justification.

§ 33. A further illustration may now be given of the subjective view of Probability at present under discussion.

An appeal to common language is always of service, as the employment of any distinct word is generally a proof that mankind have observed some distinct properties in the things, which have caused them to be singled out and have that name appropriated to them. There is such a class of words assigned by popular usage to the kind of events of which Probability takes account. If we examine them we shall find, I think, that they direct us unmistakeably to the two-fold aspect of the question,—the objective and the subjective, the quality in the events and the state of our minds in considering them,—that have occupied our attention during the former chapters.

The word 'extraordinary', for instance, seems to point to the observed fact, that events are arranged in a sort of *ordo* or rank. No one of them might be so exactly placed that we could have inferred its position, but when we take a great many into account together, running our eye, as it were, along the line, we begin to see that they really do for the most part stand in order. Those which stand away from the line have this divergence observed, and are called extraordinary, the rest ordinary, or in the line. So too 'irregular' and 'abnormal' are doubtless used from the appearance of things, when examined in large numbers, being that of an arrangement by rule or measure. This only holds when there are a good many; we could not speak of the single events being so arranged. Again the word 'law', in its philosophical sense, has now become quite popularised. How the term became introduced is not certain, but there can be little doubt that it was somewhat in this way:—The effect of a law, in its usual application to human conduct, is to produce regularity where it did not previously exist; when then a regularity began to be perceived in nature, the same word was used, whether the cause was supposed to be the same or not. In each case there was the same generality of agreement, subject to occasional deflection[1].

On the other hand, observe the words 'wonderful', 'unexpected', 'incredible'. Their connotation describes states of mind simply; they are of course not confined to Probability, in the sense of statistical frequency, but imply simply that the events they denote are such as from some cause we did not expect would happen, and at which therefore, when they do happen, we are surprised.

Now when we bear in mind that these two classes of words are in their origin perfectly distinct;—the one denoting simply events of a certain character; the other, though also denoting events, *connoting* simply states of mind;—and yet that they are universally applied to the same events, so as to be used as perfectly synonymous, we have in this a striking illustration of the two sides under which Probability may be viewed, and of the universal recognition of a close connection between them. The words are popularly used as synonymous, and we must not press their meaning too far; but if it were to be observed, as I am rather inclined to think it could, that the application of the words which denote mental states is wider than that of the others, we should have an illustration of what has been already observed, viz. that the province of Probability is not so extensive as that over which variation of belief might be observed. Probability only considers the cases in which this variation is brought about in a certain definite statistical way.

---

[1] This would still hold of *empirical* laws which may be capable of being broken: we now have very much shifted the word, to denote an *ultimate* law which it is supposed cannot be broken.

§ 34. It will be found in the end both interesting and important to have devoted some attention to this subjective side of the question. In the first place, as a mere speculative inquiry the quantity of our belief of any proposition deserves notice. To study it at all deeply would be to trespass into the province of Psychology, but it is so intimately connected with our own subject that we cannot avoid all reference to it. We therefore discuss the laws under which our expectation and surprise at isolated events increases or diminishes, so as to account for these states of mind in any individual instance, and, if necessary, to correct them when they vary from their proper amount.

But there is another more important reason than this. It is quite true that when the subjects of our discussion in any particular instance lie entirely within the province of Probability, they may be treated without any reference to our belief. We may or we may not employ this side of the question according to our pleasure. If, for example, I am asked whether it is more likely that A. B. will die this year, than that it will rain to-morrow, I may calculate the chance (which really is at bottom the same thing as my belief) of each, find them respectively, one-sixth and one-seventh, say, and therefore decide that my 'expectation' of the former is the greater, viz. that this is the more likely event. In this case the process is precisely the same whether we suppose our belief to be introduced or not; our mental state is, in fact, quite immaterial to the question. But, in other cases, it may be different. Suppose that we are comparing two things, of which one is wholly alien to Probability, in the sense that it is hopeless to attempt to assign any degree of numerical frequency to it, the only ground they have in common may be the amount of belief to which they are respectively entitled. We cannot compare the frequency of their occurrence, for one may occur too seldom to judge by, perhaps it may be unique. It has been already said, that our belief of many events rests upon a very complicated and extensive basis. My belief may be the product of many conflicting arguments, and many analogies more or less remote; these proofs themselves may have mostly faded from my mind, but they will leave their effect behind them in a weak or strong conviction. At the time, therefore, I may still be able to say, with some degree of accuracy, though a very slight degree, what amount of belief I entertain upon the subject. Now we cannot compare things that are heterogeneous; if, therefore, we are to decide between this and an event determined naturally and properly by Probability, it is impossible to appeal to chances or frequency of occurrence. The measure of belief is the only common ground, and we must therefore compare this quantity in each case. The test afforded will be an exceedingly rough one, for the reasons mentioned above, but it will be better than none; in some cases it will be found to furnish all we want.

Suppose, for example, that one letter in a million is lost in the Post Office, and that in any given instance I wish to know which is more likely, that a letter has been so lost, or that my servant has stolen it? If the latter alternative could, like the former, be stated in a numerical form, the comparison would be simple. But it cannot be reduced to this form, at least not consciously and directly. Still, if we could feel that our belief in the man's dishonesty was greater than one-millionth, we should then have homogeneous things before us, and therefore comparison would be possible.

§ 35. We are now in a position to give a tolerably accurate definition of a phrase which we have frequently been obliged to employ, or incidentally to suggest, and of which the reader may have looked for a definition already, viz. the probability of an event, or what is equivalent to this, the chance of any given event happening. I consider that these terms presuppose a series; within the indefinitely numerous class which composes this series a smaller class is distinguished by the presence or absence of some attribute or attributes, as was fully illustrated and explained in a previous chapter. These larger and smaller classes respectively are commonly spoken of as instances of the 'event,' and of 'its happening in a given particular way.' Adopting this phraseology, which with proper explanations is suitable enough, we may define the probability or chance (the terms are here regarded as synonymous) of the event happening in that particular way as the numerical fraction which represents the proportion between the two different classes in the long run. Thus, for example, let the probability be that of a given infant living to be eighty years of age. The larger series will comprise all men, the smaller all who live to eighty. Let the proportion of the former to the latter be 9 to 1; in other words, suppose that one infant in ten lives to eighty. Then the chance or probability that any given infant will live to eighty is the numerical fraction $\frac{1}{10}$. This assumes that the series are of indefinite extent, and of the kind which we have described as possessing a fixed type. If this be not the case, but the series be supposed terminable, or regularly or irregularly fluctuating, as might be the case, for instance, in a society where owing to sanitary or other causes the average longevity was steadily undergoing a change, then in so far as this is the case the series ceases to be a subject of science. What we have to do under these circumstances, is to substitute a series of the right kind for the inappropriate one presented by nature, choosing it, of course, with as little deflection as possible from the observed facts. This is nothing more than has to be done, and invariably is done, whenever natural objects are made subjects of strict science.

§ 36. A word or two of explanation may be added about the expression employed above, 'the proportion in the long run.' The run must be supposed to be very long indeed, in fact never to stop. As we keep on taking more terms of the series we shall find the proportion still fluctuating a little, but its fluctuations will grow less. The proportion, in fact, will gradually approach towards some fixed numerical value, what mathematicians term its *limit*. This fractional value is the one spoken of above. In the cases in which deductive reasoning is possible, this fraction may be obtained without direct appeal to statistics, from reasoning about the conditions under which the events occur, as was explained in the fourth chapter.

Here becomes apparent the full importance of the distinction so frequently insisted on, between the actual irregular series before us and the substituted one of calculation, and the meaning of the assertion (Ch. I. § 13), that it was in the case of the latter only that strict scientific inferences could be made. For how can we have a 'limit' in the case of those series which ultimately exhibit irregular fluctuations? When we say, for instance, that it is an even chance that a given person recovers from the cholera, the meaning of this assertion is that in the long run each alternate person attacked by that disease does recover. But if we examined a sufficiently extensive range of statistics, we might find that the manners and customs of society had produced such a change in the type of the disease or its treatment, that we were no nearer approaching towards a fixed limit than we were at first. The conception of an ultimate limit in the ratio between the numbers of the two classes in the series necessarily involves an absolute fixity of the type. When therefore nature does not present us with this absolute fixity, as she seldom or never does except in games of chance (and not demonstrably there), our only resource is to introduce such a series, in other words, as has so often been said, to substitute a series of the right kind.

§ 37. The above, which may be considered tolerably complete as a definition, might equally well have been given in the last chapter. It has been deferred however to the present place, in order to connect with it at once a proposition involving the conceptions introduced in this chapter; viz. the state of our own minds, in reference to the amount of belief we entertain in contemplating any one of the events whose probability has just been described. Reasons were given against the opinion that our belief admitted of any exact apportionment like the numerical one just mentioned. Still, it was shown that a reasonable explanation could be given of such an expression as, 'my belief is $\frac{1}{10}$th of certainty', though it was an explanation which pointed unmistakeably to a series of events, and ceased to be intelligible, or at any rate justifiable, when it was not viewed in such a relation to a series. In so far, then, as this explanation is adopted, we may say that our belief is in proportion to the above fraction. This referred to the purely intellectual part of belief which cannot be conceived to be separable, even in thought, from the things upon which it is exercised. With this intellectual part there are commonly associated various emotions. These we can to a certain extent separate, and, when separated, can measure with that degree of accuracy which is possible in the case of other emotions. They are moreover intelligible in reference to the individual events. They will be found to increase and diminish in accordance, to some extent, with the fraction which represents the scarcity of the event. The emotion of surprise does so with some degree of accuracy.

The above investigation describes, though in a very brief form, the amount of truth which appears to me to be contained in the assertion frequently made, that the fraction expressive of the probability represents also the fractional part of full certainty to which our belief of the individual event amounts. Any further analysis of the matter would seem to belong to Psychology rather than to Probability.

CHAPTER VI.

THE RULES OF INFERENCE IN PROBABILITY.

§ 1. In the previous chapter, an investigation was made into what may be called, from the analogy of Logic, Immediate Inferences. Given that nine men out of ten, of any assigned age, live to forty, what could be inferred about the prospect of life of any particular man? It was shown that, although this step was very far from being so simple as it is frequently supposed to be, and as the corresponding step really is in Logic, there was nevertheless an intelligible sense in which we might speak of the amount of our belief in any one of these 'proportional propositions,' as they may succinctly be termed, and justify that amount. We must now proceed to the consideration of inferences more properly so called, I mean inferences of the kind analogous to those which form the staple of ordinary logical treatises. In other words, having ascertained in what manner particular propositions could be inferred from the general propositions which included them, we must now examine in what cases one general proposition can be inferred from another. By a general proposition here is meant, of course, a general proposition of the statistical kind contemplated in Probability. The rules of such inference being very few and simple, their consideration will not detain us long. From the data now in our possession we are able to deduce the rules of probability given in ordinary treatises upon the science. It would be more correct to say that we are able to deduce *some* of these rules, for, as will appear on examination, they are of two very different kinds, resting on entirely distinct grounds. They might be divided into those which are formal, and those which are more or less experimental. This may be otherwise expressed by saying that, from the kind of series described in the first chapters, some rules will follow necessarily by the mere application of arithmetic; whilst others either depend upon peculiar hypotheses, or demand for their establishment continually renewed appeals to experience, and extension by the aid of the various resources of Induction. We shall confine our attention at present principally to the former class; the latter can only be fully understood when we have considered the connection of our science with Induction.

§ 2. The fundamental rules of Probability strictly so called, that is the formal rules, may be divided into two classes,—those obtained by addition or subtraction on the one hand, corresponding to what are generally termed the connection of exclusive or incompatible events[1]; and those obtained by multiplication or division, on the other hand, corresponding to what are commonly termed dependent events. We will examine these in order.

(1) We can make inferences by simple addition. If, for instance, there are two distinct properties observable in various members of the series, which properties do not occur in the same individual; it is plain that in any batch the number that are of one kind or the other will be equal to the sum of those of the two kinds separately. Thus 36.4 infants

---

[1] It might be more accurate to speak of 'incompatible hypotheses *with respect to* any individual case', or 'mutually exclusive classes of events'.

in 100 live to over sixty, 35.4 in 100 die before they are ten[1]; take a large number, say 10,000, then there will be about 3640 who live to over sixty, and about 3540 who do not reach ten; hence the total number who do not die within the assigned limits will be about 2820 altogether. Of course if these proportions were accurately assigned, the resultant sum would be equally accurate: but, as the reader knows, in Probability this proportion is merely the limit towards which the numbers tend in the long run, not the precise result assigned in any particular case. Hence we can only venture to say that this is the limit towards which we tend as the numbers become greater and greater.

This rule, in its general algebraic form, would be expressed in the language of Probability as follows:—If the chances of two exclusive or incompatible events be respectively $\frac{1}{m}$ and $\frac{1}{n}$ the chance of one or other of them happening will be $\frac{1}{m}+\frac{1}{n}$ or $\frac{m+n}{mn}$. Similarly if there were more than two events of the kind in question. On the principles adopted in this work, the rule, when thus algebraically expressed, means precisely the same thing as when it is expressed in the statistical form. It was shown at the conclusion of the last chapter that to say, for example, that the chance of a given event happening in a certain way is $\frac{1}{6}$ is only another way of saying that in the long run it does tend to happen in that way once in six times.

It is plain that a sort of corollary to this rule might be obtained, in precisely the same way, by subtraction instead of addition. Stated generally it would be as follows:—If the chance of one or other of two incompatible events be $\frac{1}{m}$ and the chance of one alone be $\frac{1}{n}$, the chance of the remaining one will be $\frac{1}{m} - \frac{1}{n}$ or $\frac{n-m}{nm}$.

---

[1] The examples, of this kind, referring to human mortality are taken from the Carlisle tables. These differ considerably, as is well known, from other tables, but *we have the* high authority of De Morgan for regarding them as the best representative of the average mortality of the English middle classes at the present *day.*

For example, if the chance of any one dying in a year is $\frac{1}{10}$, and his chance of dying of some particular disease is $\frac{1}{100}$, his chance of dying of any other disease is $\frac{9}{100}$.

The reader will remark here that there are two apparently different modes of stating this rule, according as we speak of 'one or other of two or more events happening,' or of 'the same event happening in one or other of two or more ways.' But no confusion need arise on this ground; either way of speaking is legitimate, the difference being merely verbal, and depending (as was shown in the first chapter, § 8) upon whether the distinctions between the 'ways' are or are not too deep and numerous to entitle the event to be conventionally regarded as the same.

We may also here point out the justification for the common doctrine that certainty is represented by unity, just as any given degree of probability is represented by its appropriate fraction. If the statement that an event happens once in m times, is equivalently expressed by saying that its chance is $\frac{1}{m}$, it follows that to say that it happens m times in m times, or every time without exception, is equivalent to saying that its chance is $\frac{m}{m}$ or 1. Now an event that happens *every time* is of course one of whose occurrence we are certain; hence the fraction which represents the 'chance' of an event which is certain becomes unity.

It will be equally obvious that given that the chance that an event will happen is $\frac{1}{m}$, the chance that it will not happen is $1 - \frac{1}{m}$ or $\frac{m-1}{m}$.

§ 3. (2) We can also make inferences by multiplication or division. Suppose that the two events, instead of being incompatible, are connected together in the sense that one is contingent upon the occurrence of the other. Let us be told that a given proportion of the members of the series possess a certain property, and a given proportion again of these possess another property, then the proportion of the whole which possess both properties will be found by multiplying together the two fractions which represent the above two proportions. Of the inhabitants of London, twenty-five in a thousand, say, will die in the course of the year; we suppose it to be known also that one death in five is due to fever; we should then infer that one in 200 of the inhabitants will die of fever in the course of the year. It would of course be equally simple, by division, to make a sort of converse inference. Given the total mortality per cent. of the population from fever, and the proportion of fever cases to the aggregate of other cases of mortality, we might have inferred, by dividing one fraction by the other, what was the total mortality per cent. from all causes.

The rule as given above, is variously expressed in the language of Probability. Perhaps the simplest and best statement is that it gives us the rule of dependent events, that is; If the chance of one event is $\frac{1}{m}$, and the chance that if it happens another will also happen is $\frac{1}{n}$, then the chance of the latter is $\frac{1}{mn}$. In this case it is assumed that the latter is so entirely dependent upon the former that though it does not always happen with it, it certainly will not happen without it; the necessity of this assumption however may be obviated by saying that what we are speaking of in the latter case is the *joint* event, viz. both together if they are simultaneous events, or the latter *in consequence of* the former, if they are successive.

§ 4. The above inferences are necessary, in the sense in which arithmetical inferences are necessary, and they do not demand for their establishment any arbitrary hypothesis. We assume in them no more than is warranted, and in fact necessitated by the data actually given to us, and make our inferences from these data by the help of arithmetic. In the simple examples given above nothing is required beyond arithmetic in its most familiar form, but it need hardly be added that in practice examples may often present themselves which will require much profounder methods than these. It may task all the resources of that higher and more abstract arithmetic known as algebra to extract a solution. But as the necessity of appeal to such methods as these does not touch the principles of this part of the subject we need not enter upon them here.

§ 5. The formula next to be discussed stands upon a somewhat different footing from the above in respect of its cogency and freedom from appeal to experience, or to hypothesis. In the two former instances we considered cases in which the data were supposed to be given under the conditions that the properties which distinguish the different kinds of events whose frequency was discussed, were respectively known to be disconnected and known to be connected. Let us now suppose that no such conditions are given to us.

One man in ten, say, has black hair, and one in twelve is short-sighted; what conclusions could we then draw as to the chance of any given man having one only of these two attributes, or neither, or both? It is clearly possible that the properties in question might be inconsistent with one another, so as never to be found combined in the same person; or all the short-sighted might have black hair; or the properties might be allotted[1] in almost any other proportion whatever. If we are perfectly ignorant upon these points, it would seem that no inferences whatever could be drawn about the required chances.

Inferences however *are* drawn, and practically, in most cases, quite justly drawn. An escape from the apparent indeterminateness of the problem, as above described, is found by assuming that, not merely will one-tenth of the whole number of men have black hair (for this was given as one of the data), but also that one-tenth alike of those who are and who are not short-sighted have black hair. Let us take a batch of 1200, as a sample of the whole. Now, from the data which were originally given to us, it will easily be seen that in every such batch there will be on the average 120 who have black hair, and therefore 1080 who have not. And here in strict right we ought to stop, at least until we have appealed again to experience; but we do not stop here. From data which we assume, we go on to infer that of the 120, 10 (i.e. one-twelfth of 120) will be short-sighted, and 110 (the remainder) will not. Similarly we infer that of the

---

[1] I say, *almost* any proportion, because, as may easily be seen, arithmetic imposes certain restrictions upon the assumptions that can be made. We could not, for instance, suppose that all the black-haired men are short-sighted, for in any given batch of men the former are more numerous. But the range of these restrictions is limited, and their existence is not of importance in the above discussion.

1080, 90 are short-sighted, and 990 are not. On the whole, then, the 1200 are thus divided:—black-haired short-sighted, 10; short-sighted without black hair, 90; black-haired men who are not short-sighted, 110; men who are neither short-sighted nor have black hair, 990.

This rule, expressed in its most general form, in the language of Probability, would be as follows:—If the chances of a thing being p and q are respectively $\frac{1}{m}$ and $\frac{1}{n}$, then the chance of its being both p and q is $\frac{1}{mn}$, p and not q is $\frac{n-1}{mn}$, q and not p is $\frac{m-1}{mn}$, not p and not q is $\frac{(m-1)(n-1)}{mn}$, where p and q are independent. The sum of these chances is obviously unity; as it ought to be, since one or other of the four alternatives must necessarily exist.

§ 6. I have purposely emphasized the distinction between the inference in this case, and that in the two preceding, to an extent which to many readers may seem unwarranted. But it appears to me that where a science makes use, as Probability does, of two such very distinct sources of conviction as the necessary rules of arithmetic and the merely more or less cogent ones of Induction, it is hardly possible to lay too much stress upon the distinction. Few will be prepared to deny that very arbitrary assumptions have been made by many writers on the subject, and none will deny that in the case of what are called 'inverse probabilities' assumptions are sometimes made which are at least decidedly open to question. The best course therefore is to make a pause and stringent enquiry at the point at which the possibility of such error and doubtfulness first exhibits itself. These remarks apply to some of the best writers on the subject; in the case of inferior writers, or those who appeal to

Probability without having properly mastered its principles, we may go further. It would really not be asserting too much to say that they seem to think themselves justified in assuming that where we know nothing about the distribution of the properties alluded to we must assume them to be distributed as above described, and therefore apportion our belief in the same ratio. This is called 'assuming the events to be independent,' the supposition being made that the rule will certainly follow from this independence, and that we have a right, if we know nothing to the contrary, to assume that the events are independent.

The validity of this last claim has already been discussed in the first chapter; it is only another of the attempts to construct *à priori* the series which experience will present to us, and one for which no such strong defence can be made as for the equality of heads and tails in the throws of a penny. But the meaning to be assigned to the 'independence' of the events in question demands a moment's consideration.

The circumstances of the problem are these. There are two different qualities, by the presence and absence respectively of each of which, amongst the individuals of a series, two distinct pairs of classes of these individuals are produced. For the establishment of the rule under discussion it was found that one supposition was both necessary and sufficient, namely, that the division into classes caused by each of the above distinctions should subdivide each of the classes created by the other distinction in the same ratio in which it subdivides the whole. If the independence be granted and so defined as to mean this, the rule of course will stand, but, without especial attention being drawn to the point, it does not seem that the word would naturally be so understood.

§ 7. The above, then, being the fundamental rules of inference in probability, the question at once arises, What is their relation to the great body of formulæ which are made use of in treatises upon the science, and in practical applications of it? The reply would be that these formulæ, in so far as they properly belong to the science, are nothing else in reality than applications of the above fundamental rules. Such applications may assume any degree of complexity, for owing to the difficulty of particular examples, in the form in which they actually present themselves, recourse must sometimes be made to the profoundest theorems of mathematics. Still we ought not to regard these theorems as being anything else than convenient and necessary abbreviations of arithmetical processes, which in practice have become too cumbersome to be otherwise performed.

This explanation will account for some of the rules as they are ordinarily given, but by no means for all of them. It will account for those which are demonstrable by the certain laws of arithmetic, but not for those which in reality rest only upon inductive generalizations. And it can hardly be doubted that many rules of the latter description have become associated with those of the former, so that in popular estimation they have been blended into one system, of which all the separate rules are supposed to possess a similar origin and equal certainty. Hints have already been frequently given of this tendency, but the subject is one of such extreme importance that a separate chapter (that on Induction) must be devoted to its consideration.

§ 8. In establishing the validity of the above rules, we have taken as the basis of our investigations, in accordance with the general scheme of this work, the statistical frequency of the events referred to; but it was also shown that each formula, when established, might with equal propriety be expressed in the more familiar form of a fraction representing the 'chance' of the occurrence of the particular event. The question may therefore now be raised, Can those writers who (as described in the last chapter) take as the primary subject of the science not the degree of statistical frequency, but the quantity of belief, with equal consistency make this the basis of their rules, and so also regard the fraction expressive of the chance as a merely synonymous expression? De Morgan maintains that whereas in ordinary logic we suppose the premises to be absolutely true, the province of Probability is to study 'the effect which partial belief of the premises produces with respect to the conclusion.' It would appear therefore as if in strictness we ought on this view to be able to determine this consequent diminution at first hand, from introspection of the mind, that is of the conceptions and beliefs which it entertains; instead of making any recourse to statistics to tell us how much we ought to believe the conclusion.

Any readers, who have concurred with me in the general results of the last chapter, will naturally agree in the conclusion that nothing deserving the name of logical science can be extracted from any results of appeal to our consciousness as to the quantity of belief we entertain of this or that proposition. Suppose, for example, that one person in 100 dies on the sea passage out to India, and that one in 9 dies during a 5 years residence there. It would commonly be said that the chance that any one who is now going out, has of living to start on his return 5 years hence, is $\frac{88}{100}$; for his chance of getting there is $\frac{99}{100}$ and of his surviving, if he gets there, $\frac{8}{9}$; hence the result or dependent event is got by multiplying these fractions together, which gives $\frac{88}{100}$. Here the real basis of the reasoning is statistical, and the processes or results are merely translated afterwards into fractions. But can we say the same when we look at the belief side of the question? I quite admit the psychological fact that we have degrees of belief, more or less corresponding to the frequency of the events to which they refer. In the above example, for instance, we should undoubtedly admit on enquiry that our belief in the man's return was affected by each of the risks in question, so that we had less expectation of it than if he were subject to either risk separately; that is we should in some way compound the risks. But what I cannot recognize is that we should be able to perform the process with any approach to accuracy without appeal to the statistics, or that, even supposing we could do so, we should have any possible guarantee of the correctness of the result without similar appeal. It appears to me in fact that but little meaning, and certainly no security, can be attained by so regarding the process of inference. The probabilities expressed as degrees of belief, just as those which are expressed as fractions, must, when we are put upon our justification, first be translated into their corresponding facts of statistical frequency of occurrence of the events, and then the inferences must be drawn and justified there. This part of the operation, as we have already shown, is mostly carried on by the ordinary rules of arithmetic. When we have obtained our conclusion we may, if we please, translate it back again into the subjective form, just as we can and do for convenience into the fractional, but I do not see how the process of inference can be conceived as taking place in that form, and still less how any proof of it can thus be given. If therefore the process of inference be so expressed it must be regarded as a symbolical process, symbolical of such an inference about things as has been described above, and it therefore seems to me more advisable to state and expound it in this latter form.

On Inverse Probability and the Rules required for it.

§ 9. It has been already stated that the only fundamental rules of Probability are the two described in §§ 2, 3, but there are of course abundance of derivative rules, the nature and use of which are best obtained from the study of any manual upon the subject. One class of these derivative rules, however, is sufficiently distinct in respect of the questions to which it may give rise, to deserve somewhat special examination. It involves the distinction commonly recognised as that between Direct and Inverse Probability. It is thus introduced by De Morgan:—

"In the preceding chapter we have calculated the chances of an event, knowing the circumstances under which it is to happen or fail. We are now to place ourselves in an inverted position: we know the event, and ask what is the probability which results from the event in favour of any set of circumstances under which the same might have happened[1]." The distinction might therefore be summarily described as that between finding an effect when we are given the causes, and finding a cause when we are given effects.

On the principles of the science involved in the definition which was discussed and adopted in the earlier chapters of this work, the reader will easily infer that no such distinction as this can be regarded as fundamental. One common feature was traced in all the objects which were to be referred to Probability, and from this feature the possible rules of inference can be immediately derived. All other distinctions are merely of arrangement or management.

---

[1] *Essay on Probabilities*, p. 53. I have been reminded that in his article on Probability in the *Encyclopædia Metropolitana* he has stated that such rules involve no new principle.

But although the distinction is not by any means fundamental, it is nevertheless true that the practical treatment of such problems as those principally occurring in Inverse Probability, does correspond to a very serious source of ambiguity and perplexity. The arbitrary assumptions which appear in Direct Probability are not by any means numerous, and may by the exercise of a little firmness, be almost entirely obviated; but those which invade us in a large proportion of the problems offered by Inverse Probability are of the very essence of the subject.

§ 10. This will be best seen by the examination of special examples; as any, however simple, will serve our purpose, let us take the two following:—

(1) A ball is drawn from a bag containing nine black balls and one white: what is the chance of its being the white ball?

(2) A ball is drawn from a bag containing ten balls, and is found to be white; what is the chance of there having been but that one white ball in the bag?

The class of which the first example is a simple instance has been already abundantly discussed. The interpretation of it is as follows: If balls be continually drawn and replaced, the proportion of white ones to the whole number drawn will tend towards the fraction $\frac{1}{10}$. The contemplated action is a single one, but we view it as one of the above series; at least our opinion is formed upon that assumption. We conclude that we are going to take one of a series of events which may appear individually fortuitous, but in which, in the long run, those of a given kind are one-tenth of the whole; this kind (white) is then singled out by anticipation. By stating that its chance is $\frac{1}{10}$, we merely mean to assert this physical fact, together with such other mental facts, emotions, inferences, &c., as may be properly associated with it.

§ 11. Have we to interpret the second example in a different way? Here also we have a single instance, but the nature of the question would seem to decide that the only series to which it can properly be referred is the following:—Balls are continually drawn from *different* bags each containing ten, and are always found to be white; what is ultimately the proportion of cases in which they will be found to have been taken from bags with only one white ball in them? Now it may be readily shown[1] that time has nothing to do with the question; omitting therefore the consideration of this element, we have for the two series from which our opinions in these two examples respectively are to be formed:—(1) balls of different colours presented to us in a given ultimate ratio; (2) bags with different contents similarly presented. From these data respectively we have to assign their due weight to our anticipations of (1) a white ball; (2) a bag containing but one white ball. So stated the problems would appear to be formally identical, the only difference being in the matter. Theory therefore would recognise no distinction of principle between them.

When, however, we begin the practical work of solving them we perceive a most important distinction. In the first example there is not much that is arbitrary; balls would under such circumstance really come out more or less accurately in the proportion expected. Moreover, in case it should be objected that it is difficult to prove that they will do so, it does not seem an unfair demand to say that the balls are to be 'well-mixed' or 'fairly distributed,' or to introduce any of the other conditions by which, under the semblance of judging *à priori*, we take care to secure our prospect of a series of the desired kind. But we cannot say the same in the case of the second example.

---

[1] This point will be more fully discussed in the eleventh chapter, after the general stand-point of an objective system of logic has been explained and illustrated.

§ 12. The line of proof by which it is generally attempted to solve the second example is of this kind;—It is shown that there being one white ball for certain in the bag, the only possible antecedents are of ten kinds, viz. bags, each of which contains ten balls, but in which the white balls range respectively from one to ten in number. This of course imposes limits upon the kind of terms to be found in our series. But we want more than such limitations, we must know the proportions in which these terms are ultimately found to arrange themselves in the series. Now this requires an experience about bags which may not, and indeed in a large proportion of similar cases, cannot, be given to us. If therefore we are to solve the question at all we must make an assumption; let us make the following;—*that each of the bags described above occurs equally often*,—and see what follows. The bags being drawn from equally often, it does not follow that they will each yield equal numbers of white balls. On the contrary they will, as in the last example, yield them in direct proportion to the number of such balls which they contain. The bag with one white and nine black will yield a white ball once in ten times; that with two white, twice; and so on. The result of this, it will be easily seen, is that in 100 drawings there will be obtained on the average 55 white balls and 45 black. Now with those drawings that do not yield white balls we have, by the question, nothing to do, for that question, as it was originally stated, postulated the drawing of a white ball as an accomplished fact. The series we want is therefore composed of

1
2
3
4
5
6
7
8
9
10
55

those which do yield white. Now what is the additional attribute which is found in some members, and in some members only, of this series, and which we mentally anticipate? Clearly it is the attribute of having been drawn from a bag which only contained one of these white balls. Of these there is, out of the 55 drawings, but one. Accordingly the required chance is $\frac{1}{55}$. That is to say, the white ball will have been drawn from the bag containing only that one white, once in 55 times.

§ 13. Now, with the exception of the passage in italics, the process here is precisely the same as in the other example; it is somewhat longer only because we are not able to appeal immediately to experience, but are forced to try to deduce what the result will be, though the validity of this deduction itself rests, of course, ultimately upon experience. But the above passage is a very important one. It is scarcely necessary to point out how entirely arbitrary it is. The nature of the assumption is commonly disguised by saying that, where we have no reason to expect one kind of bag rather than another it is only reasonable to regard all possible kinds as equally likely. But, as has been before insisted, this phrase 'equally likely' is one that must submit itself to explanation. If any one replies that he means nothing more by it than that he expects the events equally, he is at liberty to do so; but he should remember that he is then applying the words entirely to his state of mind, and thereby unavoidably admitting that they have no necessary connection with experience. If they are to have such a connection he can only mean that the events do happen equally often; for our opinion about the events being equally likely is of little value, indeed should rather be called a guess, unless we have good reason to believe that it is really in harmony with experience. Now will any one say that in his experience in this world bags of the kind described above occur with equal frequency? or even that it is his deliberate conviction that they will do so? We may assume, as in the common hypothesis, that such are the conditions under which the course of nature is carried on; but it is best that the assumption should be openly proclaimed.

A few minutes consideration of the assumption italicised above will convince the reader that those complaints are not exaggerated. *Is* the supposition, that the different specified kinds of bags are equally likely, the most reasonable supposition under the circumstances in question? One man may think it is, another may take a contrary view. In fact in an excellent manual[1] upon the subject a totally different supposition is made, at any rate in one example; it is taken for granted in that instance, not that every possible number of black and white balls respectively is equally likely, but that every possible way of getting each number is equally likely, whence it follows that bags with an intermediate number of black and white balls are far more likely than those with an extreme number of either. On this supposition five black and five white being obtainable in 252 ways against the ten ways of obtaining one white and nine black, it follows that the chance that we have drawn from a bag of the latter description is much less than on the hypothesis first made. The chance, in fact, becomes now $\frac{1}{512}$ instead of $\frac{1}{55}$. In the one case each distinct result is considered equally likely, in the other every distinct way of getting each result.

§ 14. Uncertainties of this kind are peculiarly likely to arise in these inverse probabilities, because when we are merely given our effect and told to look out for the chance of some assigned cause, we are often given no clue as to the relative prevalence of these causes, but are left to determine them on general principles. Give us either their actual prevalence in statistics, or the conditions by which such prevalence is brought about, and we know what to do; but without the help of such data we are reduced to guessing in the wildest uncertainty. In the above example, if we had been told how the bag had been originally filled, that is by what process, or under what circumstances, we should have known what to do. If it had been filled at random from a box containing equal numbers of black and white balls, the supposition in Mr Whitworth's example is the most reasonable; but in the absence of any such information as this we are entirely in the dark, and the supposition made in § 12 is neither more nor less trustworthy and reasonable than many others, though it doubtless possesses the merit of superior simplicity[1].

---

[1] Whitworth's *Choice and Chance*, Ed. II., p. 123. See also Boole's *Laws of Thought*, p. 370.

§ 15. When, however, we take instead what may be called, by comparison with the above purely artificial examples, instances presented by nature, much of this uncertainty will disappear or may be avoided, and then all real distinction between direct and inverse probability vanishes. In such cases the causes are mostly determined by tolerably definite rules, instead of being a mere cloud-land of capricious guesses. We may either find their relative frequency of occurrence by reference to tables, or may be able to infer it by examination of the circumstances under which they are brought about. Almost any simple example would then serve to illustrate the fact that under such circumstances the distinction between direct and inverse probability disappears altogether, or merely resolves itself into one of *time*, which, as will be more fully shown in a future chapter, is entirely foreign to our subject.

---

[1] Opinions differ about the defence of such suppositions, as they do about the nature of them. Some writers, admitting the above assumption to be unjustifiable, call it the most impartial hypothesis. Others regard it as a sort of mean hypothesis.

It is not of course intended to imply that difficulties similar to those mentioned above do not occasionally invade us here also. As already mentioned, they are, if not inherent in the subject, at any rate almost unavoidable in comparison with the simpler and more direct procedure of determining what is likely to follow from assigned conditions. What is meant is that so long as we confine ourselves within the comparatively regular and uniform field of natural sequences and coexistences, statistics of causes may be just as readily available as those of effects. There will not be much more that is arbitrary in the one than in the other. But of course this security is lost when, as will be almost immediately noticed, what may be called metaphysical rather than natural causes are introduced into the enquiry.

§ 16. Considered therefore as a contribution to the theory of the subject, the distinction between Direct and Inverse Probability must be abandoned. When the appropriate statistics are at hand the two classes of problems become identical in method of treatment, and when they are not we have no more right to extract a solution in one case than in the other. The discussion however may serve to direct renewed attention to another and far more important distinction. It will remind us that there is one class of examples to which the calculus of Probability is rightfully applied, because statistical data are all we have to judge by; whereas there are other examples in regard to which, if we will insist upon making use of these rules, we may either be deliberately abandoning the opportunity of getting far more trustworthy information by other means, or we may be obtaining solutions about matters on which the human intellect has no right to a definite opinion at all.

§ 17. Examples of the general description mentioned above seem objectionable enough, but if we wish to realize to its full extent the vagueness of some of the problems submitted to this Inverse Probability, we have not far to seek. In natural as in artificial examples, where statistics are unattainable the enquiry becomes utterly hopeless, and all attempts at laying down rules for calculation must be abandoned. Take, for instance, the question which has given rise to some discussion[1], whether such and such groups of stars are or are not to be regarded as the results of an accidental distribution; or the still wider and vaguer question, whether such and such things, or say the world itself, have been produced by chance?

In cases of this kind the insuperable difficulty is in determining what sense exactly is to be attached to the words 'accidental' and 'random' which enter into the discussion. Some brief account was given of their conventional meaning in Probability, at the end of the third chapter. There seem to be the same objections to generalizing them out of such relation, as there is in metaphysics to talking of the Infinite or the Absolute. Infinite magnitude, or infinite power, one can to some extent comprehend, or at least one may understand what is being talked about, but '*the* infinite' seems a term devoid of meaning. So of anything supposed to have been produced at random: tell us the nature of the agency, the limits of its randomness and so on, and we can venture upon the problem, but without such data we know not what to do. The further consideration of such a problem might, I think, without arrogance be relegated to the Chapter on Fallacies. Accordingly any further remarks which I have to make upon the subject will be found there, and at the conclusion of the chapter on Causation and Design.

---

[1] See Todhunter's *History*, pp. 333, 4.

There is an interesting discussion upon this question by J. D. Forbes in a well-known paper in the *Philosophical Magazine* for Dec. 1850.

CHAPTER VII.

THE RULE OF SUCCESSION.

§ 1. In the last chapter we discussed at some length the nature of the kinds of inference in Probability which correspond to those termed in Logic immediate and mediate inferences. We ascertained what was the meaning of saying, for example, that the chance of any given man A. B. dying in a year is $\frac{1}{3}$, when concluded from the general proposition that one man out of three in his circumstances dies. We also discussed the nature and evidence of rules of a more completely inferential character. But to stop at this point would be to take a very imperfect view of the subject. If Probability is a science of real inference about things, it must surely lead up to something more than such merely formal conclusions; we must be able, if not by means of it, at any rate by some means, to step beyond the limits of what has been actually observed, and to draw conclusions about what is as yet unobserved. This leads at once to the question, What is the connection of Probability with Induction? This is a question into which it will be necessary to enter now with some minuteness.

That there is a close connection between Probability and Induction, must have been observed by almost every one who has treated of either subject; I have not however seen any account of this connection that seemed to me to be satisfactory. An explicit description of it should rather be sought in treatises upon the narrower subject, Probability; but it is precisely here that the most confusion is to be found. The province of Probability being somewhat narrow, incursions have been constantly made from it into the adjacent territory of Induction. In this way, amongst the arithmetical rules discussed in the last chapter, others have been frequently introduced which ought not in strictness to be classed with them, as they rest on an entirely different basis.

§ 2. The origin of such confusion is easy of explanation; it arises, doubtless, from the habit of laying undue stress upon the *subjective* side of Probability, upon that which treats of the quantity of our belief upon different subjects and the variations of which that quantity is susceptible. It has been already urged that this variation of belief is at most but an invariable accompaniment of what is really essential to Probability, and is moreover common to other subjects as well. By defining the science therefore from this side these other subjects would claim admittance into it; some of these, as Induction, have been accepted, but others have been somewhat arbitrarily rejected. Our belief in a wider proposition gained by Induction is, prior to verification, not so strong as that of the narrower generalization from which it is inferred. This being observed, a so-called rule of probability has been given by which it is supposed that this diminution of assent could in many instances be calculated.

But *time* also works changes in our conviction; our belief in the happening of almost every event, if we recur to it long afterwards, when the evidence has faded from the mind, is less strong than it was at the time. Why are not rules of oblivion inserted in treatises upon Probability? If a man is told how firmly he ought to expect the tide to rise again, because it has already risen ten times, might he not also ask for a rule which should tell him how firm should be his belief of an event which rests upon a ten years' recollection? The infractions of a rule of this latter kind could scarcely be more numerous and extensive, as we shall see presently, than those of the former confessedly are. The fact is that the agencies, by which the strength of our conviction is modified, are so indefinitely numerous that they cannot all be assembled into one science; for purposes of definition therefore the quantity of belief had better be omitted from consideration, or at any rate regarded as a mere appendage, and the science defined from the other or statistical side of the subject, in which, as has been shown, a tolerably clear boundary-line can be traced.

§ 3. Induction, however, from its importance does merit a separate discussion; a single example will show its bearing upon this part of our subject. We are considering the prospect of a given man, A. B., living another year, and we find that nine out of ten men of his age do survive. In forming an opinion about his surviving, however, we shall find that there are in reality two very distinct causes which aid in determining the strength of our conviction; distinct, but in practice so intimately connected that we are very apt to overlook one, and attribute the effect entirely to the other.

(I) There is that which strictly belongs to Probability; that which (as was explained in Chap. V.) measures our belief of the individual case as deduced from the general proposition. Granted that nine men out of ten of the kind to which A. B. belongs do live another year, it obviously does not follow at all necessarily that *he* will. We describe this state of things by saying, that our belief of his surviving is diminished from certainty in the ratio of 10 to 9, or, in other words, is measured by the fraction $\frac{9}{10}$.

(II) But are we certain that nine men out of ten like him *will* live another year? we know that they have so survived in time past, but will they continue to do so? Since A. B. is still alive it is plain that this proposition is to a certain extent assumed, or rather obtained by Induction. We cannot however be as certain of the inductive inference as we are of the data from which it was inferred. Here, therefore, is a second cause which tends to diminish our belief; in practice these two causes always accompany each other, but in thought they can be separated.

The two distinct causes described above are very liable to be confused together, and the class of cases from which examples are necessarily for the most part drawn increases this liability. The step from the statement 'all men have died in a certain proportion' to the inference 'they will continue to die in that proportion' is so slight a step that it is unnoticed, and the diminution of conviction that should accompany it is unsuspected. In what are called *à priori* examples the step is still slighter. We feel so certain about the permanence of the laws of mechanics, that few people would think of regarding it as an inference when they believe that a die will in the long run turn up all its faces equally often, because other dice have done so in time past.

§ 4. It has been already pointed out (in Chapter V.) that, so far as concerns that definition of Probability which regards it as the science which discusses the degree and modifications of our belief, the question at issue seems to be simply this:—Are the causes alluded to above in (II) capable of being reduced to one simple coherent scheme, so that any universal rules for the modification of assent can be obtained from them? If they are, strong grounds will have been shown for classing them with (I), in other words, for considering them as rules of probability. Even then they would be rules practically of a very different kind, contingent instead of necessary (if one may use these terms without committing oneself to any philosophical system), but this objection might perhaps be overruled by the greater simplicity secured by classing them together. This view is, with various modifications, generally adopted by writers on Probability, or at least, as I understand the matter, implied by their methods of definition and treatment. Or, on the other hand, must these causes be regarded as a vast system, one might almost say a chaos, of perfectly distinct agencies; which may indeed be classified and arranged to some extent, but from which we can never hope to obtain any rules of perfect generality which shall not be subject to constant exception? If so, but one course is left; to exclude them all alike from Probability. In other words, we must assume the general proposition, viz. that which has been described throughout as our starting-point, to be given to us; it may be obtained by any of the numerous rules furnished by Induction, or it may be inferred deductively, or given by our own observation; its value may be diminished by its depending upon the testimony of witnesses, or its being recalled by our own memory. Its real value may be influenced by these causes or any combinations of them; but all these are preliminary questions with which we have nothing directly to do. We assume our statistical proposition to be true, neglecting the diminution of its value by the process of attainment; we take it up first at this point and then apply our rules to it. We receive it in fact, if one may use the expression, *ready-made*, and ask no questions about the process or completeness of its manufacture.

§ 5. It is not to be supposed, of course, that any writers have seriously attempted to reduce to one system of calculation all the causes mentioned above, and to embrace in one formula the diminution of certainty to which the inclusion of them subjects us. But on the other hand, they have been unwilling to restrain themselves from all appeal to them. From an early period in the study of the science attempts have been made to proceed, by the Calculus of Probability, from the observed cases to adjacent and similar cases. In practice, as has been already said, it is not possible to avoid some extension of this kind. But it should be observed, that in these instances the divergence from the strict ground of experience is not in reality recognized, not, that is, as a part of our logical procedure. We have, it is true, wandered somewhat beyond it, and so obtained a wider proposition than our data strictly necessitated, and therefore one of less certainty. Still we assume the conclusion given by induction to be equally certain with the data, or rather omit all notice of the divergence from consideration. It is assumed that the unexamined instances will resemble the examined, an assumption for which abundant warrant may exist; the theory of the calculation rests upon the supposition that there will be no difference between them, and the practical error is insignificant simply because this difference is small.

§ 6. But the rule we are now about to discuss, and which may be called the Rule of Succession, is of a very different kind. It not only recognizes the fact that we are leaving the ground of past experience, but takes the consequences of this divergence as the express subject of its calculation. It professes to give a general rule for the measure of expectation that we should have of the reappearance of a phenomenon that has been already observed any number of times. This rule is generally stated somewhat as follows: "To find the chance of the recurrence of an event already observed, divide the number of times the event has been observed, increased by one, by the same number increased by two."

It must be confessed that this rule has been received in a thankless spirit. For considering what a number of events there are in the world, and how many are the ways in which they may happen, there is certainly no reason to fear that there will long be any want of occasions on which to appeal to the rule. The truth of it does not seem to be doubted by the principal writers on Probability[1]; whilst those who have obtained their results from the mathematicians, as Archbishop Thomson, in his *Laws of Thought*, seem to regard it as standing on precisely the same footing as any of the other rules of the Science.

§ 7. It is necessary to give some criticism of this rule, both from the general acceptance it has received, and from the eminence of many of its supporters. Moreover, however much the rule itself may be found to fail when examined, a few principles of inference of real importance will, it is hoped, be elicited in the course of our investigation. This will serve as an introduction to the treatment of Induction as it stands related to Probability. We will afterwards shift the enquiry on to somewhat broader grounds, and examine some of the problems which have to be met in the case of any attempt to lay down rules of proof and discovery. These enquiries do not very strictly belong to Probability, but they are so constantly encountered in the discussions which arise there, that it seems essential to clear our way towards forming a decided opinion upon them.

Now there is one view of the question which deserves a passing notice, but nothing more. We may presume that the eminent writers who have accepted this rule do not regard it as the expression of a mere brute instinct. It is conceivable that on Physiological or Psychological grounds, the mere repetition of an event a certain number of times should excite a growing expectation of its recurrence. Employing an illustration that shall at least have some connection with the derivation of the word, our impressions might be like those produced by hitting some soft inelastic substance with a mallet, where each successive blow deepens the impression. This would be to regard the rule as merely the expression of a mental instinct. But if it is to be considered as supplying real inferences about things, we cannot rest here. An instinctive belief may need to be corrected, to bring it into accordance with experience; and if not, it must at least submit to justify itself by experience. It has been repeatedly stated already that to tell a rational being that his expectation of an event should be, say, $\frac{3}{4}$, can mean nothing else at bottom than this;—that events of the kind contemplated do really happen in that way three times out of four.

---

[1] Boole is a prominent exception. See his *Laws of Thought*, p. 370.

It is quite true that there must be some mental link to bind together the examined and the unexamined cases, before we can venture upon any new inferences about the latter. Without such a link there could of course be no extension beyond the strict limits of past experience. It is also obvious that a stronger degree of conviction or anticipation is produced in some instances than in others, the strength of this conviction depending unquestionably, in part, upon the degree of resemblance between the examined and the unexamined cases; it also depends in part upon the number of times in which the examined cases have been already observed to occur. But it need not be the case, as seems to be very commonly supposed, that the conviction must increase uniformly from indifference towards certainty in proportion to the number of these observed instances of recurrence.

It is at least possible, as is held by some writers[1], that the conviction should exist in its full degree after the *first* occurrence of the event, viz. that our primitive impulse should be to fully believe that any two things which had been once observed together would so occur again; this belief becoming of course altered in amount, or, it may be, shaken and subverted by subsequent experience.

But whichever way the matter be settled, it is not easy to see how such considerations can have any bearing upon rules of inference at the point at which they are taken up in Logic or Probability. The psychological principles just mentioned lie at an immense depth below the surface of these rules, and assume a very different form before they emerge into the shape of laws of inference for minds of mature intelligence. In this latter shape they must, of course, submit to be tested by experience, as we have been prepared to test them throughout; but I cannot see that we are concerned with the process of their growth, or the germ out of which they have been developed. This side of the question will however recur to our notice in the course of the next chapter.

I find it difficult to ascertain precisely from Laplace's Essay what his view of this Rule of Succession is. On the one hand he certainly appeals to it as a valid rule of inference, but on the other hand he enters into decidedly psychological and even physiological explanations in the latter part of his Essay. But he does not appear to perceive the fact that by converting any such formula into an ultimate principle we do in reality abandon it as a practical rule.

§ 8. Is the rule then really true? Let us appeal to experience. In order that there shall be no unfairness we will begin with one or two examples selected by some of the most eminent writers on the subject. Laplace has ascertained that, at the date of the writing of his work, one might have safely betted 1826214 to 1 in favour of the sun's rising again. Since then however time has justified us in laying longer odds. De Morgan says, that a man who standing on the bank of a river has seen ten ships pass by with flags, should judge it to be 11 to 1 that the next ship will also carry a flag. Let us add an example or two more of our own. I have observed it rain three days successively,—I have found on three separate occasions that to give my fowls strychnine has caused their death,—I have given a false alarm of fire on three different occasions and found the people come to help me each time. In each of these latter cases, then, I am to form an opinion of just the intensity of $\frac{4}{5}$ in favour of a repetition of the phenomenon under similar circumstances. But no one, we may presume, will assert that in any one of these cases the opinion so formed would be correct. In some of them our expectation would have been overrated, in some immensely underrated. By calling the expectation wrong, it is not merely meant that it is frustrated in the particular case in question; this kind of failure, of course, is to be looked for in questions of Probability. But it is wrong in the long run. No amount of repetition in our appeals to the rule in similar cases would lead us to even an average truth, and for this latter kind of truth we have a right to look. With one single exception, and that a very doubtful one (the case of games of chance, bags and balls[1], &c.), the same objection could probably be brought against every possible application of this rule. Now granting that a formula of this kind being given to any one, he might be

---

[1] "The leading fact in Belief, according to my view of it, is our Primitive Credulity."—BAIN, *Emotions and Will*, Ed. III. p. 511.

$\frac{3+1}{3+2}$

---

[1] The 'proof' of the rule is of course sought, with the aid of various assumptions, in the inferences we should draw as to the contents of a bag with balls in it.

justified in making use of it for a time, surely as soon as he has tried it and repeatedly found it lead him astray, it becomes his duty to reject and denounce it for the future. If, in the above examples, a person has really proportioned his expectation in the manner described, and has afterwards discovered by the examination of other instances of the same class, as he could hardly fail to do, that his opinion had been grossly wrong, is he still to adhere to the rule for the future? We need not blame him for doing what he did at the time; he might have known no better; but is he to let the rule be published to the world as a true one?

§ 9. It is surely a mere evasion of the difficulty to assert, as is sometimes done, that the rule is to be employed in those cases only in which we do not know anything beforehand about the mode and frequency of occurrence of the events. The truth or falsity of the rule cannot be in any way dependent upon the ignorance of the man who uses it. His ignorance affects himself only, and corresponds to no distinction in the things. In reality the two classes, viz. of cases in which we have and have not some preliminary information, are for the most part identical; not, of course, identical to the same person at the same time, but in the sense that what one person does not know at present, he may hereafter, and others do know now. To say therefore that the rule refers to cases where there is no such preliminary information, is irrelevant when the question is as to the correctness of the rule. We cannot fling the rule amongst mankind with the prescription attached that it is merely to be taken by the ignorant. It is of course a common enough thing to have to be content with merely approximate results, or to be forced to employ a rule which we know will sometimes fail, but the case before us represents a far more serious defect than this. A formula is given to us which really does not seem fitted to put those who trust to it oftener in the right path than if they had never heard of it.

§ 10. It has been said above that the truth of the Rule of Succession seems seldom to be doubted by mathematical writers on the subject. From this statement, however, exceptions must be made. Prof. De Morgan is far too acute and philosophical a writer to accept the rule with the blind confidence with which it has sometimes been received. He regards it as furnishing a *minimum* value for the amount of our expectation[1].

He would appear therefore to recognize only the instances in which our belief in the uniformity of nature, and in the existence of special laws of causation, comes in to aid that which we should entertain from the mere frequency of past occurrence of the particular event in question. His opinion is one from which every one who writes upon this subject must dissent with deference, but it certainly appears to me to be irreconcilable with some of the instances given above. We have seen that there are cases in which the fact of a thing having already happened several times is a strong reason *against* it happening again. Can any marks be given by which these particular cases should be detected beforehand? and, if not, how can we assign a minimum value to the formula? A false alarm given several times in succession is no unfair specimen of a considerable class of recurrences (others will be given in Chap. XIV). Whilst such cases exist, it is not easy to see how the rule can be regarded as correct.

§ 11. The Rule has been discussed, during this chapter, in its simplest form. The above criticisms, however, will apply with equal or greater force to the somewhat more complex form, in which it is attempted to determine the chance, not of one more recurrence only, but of any number of recurrences.

---

[1] He terms it "The rule of probability of a *pure induction*," and says of it: "The probabilities shown by the above rules are merely *minima*, which may be augmented by other sources of knowledge." (*Formal Logic*, p. 215.)

So much then for this Rule of Succession. Not that we have yet exhausted its shortcomings; for, as we shall see presently, it does not merely mislead us by giving one determinate but incorrect answer; it perplexes us by the offer of several discordant and often contradictory answers, all of them presumably incorrect. Nothing but the celebrity of its supporters, and the wide acceptance it has met with, have been our reasons for examining it so minutely as we have done.

But to simply criticise and reject it is not sufficient. We should like to know somewhat more fully *why* it fails so utterly, and whether anything can be substituted for it; for the end which it seeks, viz. the extension of our inference beyond the limits of direct observation, is one which is desirable, and indeed necessary if we are ever to obtain information about things in general. The rule, as has been already said, seems to involve considerable confusion between Probability and Induction. This confusion can only be resolved, and the portion of truth mixed up in it elicited, by trying back some steps, and commencing with an analysis of the province and nature of Induction. This is the task to which we shall proceed in the course of the next chapter.

CHAPTER VIII.

INDUCTION AND ITS CONNECTION WITH PROBABILITY.

§ 1. We were occupied, during the last chapter, with the examination of a rule, the object of which was to enable us to make inferences about instances as yet unexamined. It was professedly, therefore, a rule of an inductive character. But, in the form in which it is commonly expressed, it was found to fail utterly, proving, when applied to the phenomena of nature, to be generally at least false or inapplicable. It is reasonable therefore to enquire at this point whether Probability is entirely a formal or deductive science, or whether, on the other hand, we are able, by means of it, to make valid inferences about instances as yet unexamined. This question has been already in part answered by implication in the course of the last two chapters. It is proposed in the present chapter to devote a fuller investigation to this subject, and to describe, as minutely as limits will allow, the nature of the connection between Probability and Induction. We shall find it advisable for clearness of conception to commence our enquiry at a somewhat early stage. We will travel over the ground, however, as rapidly as possible, until we approach the boundary of what can properly be termed Probability.

§ 2. Let us then conceive some one setting to work to investigate nature, under its broadest aspect, with the view of systematizing the facts of experience that are known, and thence (in case he should find that this is possible) discovering others which are at present unknown. He observes a multitude of phenomena, physical and mental, contemporary and successive. He enquires what connections are there between them? what rules can be found, so that some of these things being observed I can infer others from them? We suppose him, let it be observed, deliberately resolving to investigate the things themselves, and not to be turned aside by any prior enquiry as to there being laws under which the mind is compelled to judge of the things. This may arise either from a disbelief in the existence of any independent and necessary mental laws, and a consequent conviction that the mind is perfectly competent to observe and believe anything that experience offers, and should believe nothing else, or simply from a preference for investigations of the latter kind. In other words, we suppose him to reject Formal Logic, and to apply himself to a study of objective existences.

It must not for a moment be supposed that we are here doing more than conceiving a fictitious case for the purpose of more vividly setting before the reader the nature of the inductive process, the assumptions it has to make, and the character of the materials to which it is applied. It is not psychologically possible that any one should come to the study of nature with all his mental faculties in full perfection, but void of all materials of knowledge, and free from any bias as to the uniformities which might be found to prevail around him. In practice, of course, the form and the matter—the laws of belief or association, and the objects to which they are applied—act and react upon one another, and neither can exist in any but a low degree without presupposing the existence of the other. But the supposition is perfectly legitimate for the purpose of calling attention to the requirements of such a system of Logic, and is indeed nothing more than what has to be done at almost every step in psychological enquiry[1].

§ 3. His task at first might be conceived to be a slow and tedious one. It would consist of a gradual accumulation of individual instances, as marked out from one another by various points of distinction, and connected with one another by points of resemblance. These would have to be respectively distinguished and associated in the mind, and the consequent results would then be summed up in general propositions, from which inferences could afterwards be drawn. These inferences could, of course, contain no new facts, they would only be repetitions of what he or others had previously observed. All that we should have so far done would have been to make our classifications of things and then to appeal to them again. We should therefore be keeping well within the province of ordinary logic, the processes of which (whatever their ultimate explanation) may of course always be expressed, in accordance with Aristotle's Dictum, as ways of determining whether or not we can show that one given class is included wholly or partly within another, or excluded from it, as the case may be.

§ 4. But a very short course of observation would suggest the possibility of a wide extension of his information. Experience itself would soon detect that events were connected together in a regular way; he would ascertain that there are 'laws of nature.' Coming with no *à priori* necessity of believing in them, he would soon find that as a matter of fact they do exist, though he could not feel any certainty as to the extent of their prevalence. The discovery of this arrangement in nature would at once alter the plan of his proceedings, and set the tone to the whole range of his methods of investigation. His main work now would be to find out by what means he could best discover these laws of nature.

---

[1] Some of my readers may be familiar with a very striking digression in Buffon's Natural History (*Natural Hist. of Man*, § VIII.), in which he supposes the first man in full possession of his faculties, but with all his experience to gain, and speculates on the gradual acquisition of his knowledge. Whatever may be thought of his particular conclusions the passage is very interesting and suggestive to any student of Psychology.

An illustration may assist. Suppose I were engaged in breaking up a vast piece of rock, say slate, into small pieces. I should begin by wearily working through it inch by inch. But I should soon find the process completely changed owing to the existence of *cleavage*. By this arrangement of things a very few blows would do the work—not, as I might possibly have at first supposed, to the extent of a few inches—but right through the whole mass. In other words, by the process itself of cutting, as shown in experience, and by nothing else, a constitution would be detected in the things that would make that process vastly more easy and extensive. Such a discovery would of course change our tactics. Our principal object would thenceforth be to ascertain the extent and direction of this cleavage.

Something resembling this is found in Induction. The discovery of laws of nature enables the mind to dart with its inferences from a few facts completely through a whole class of objects, and thus to acquire results the successive individual attainment of which would have involved long and wearisome investigation, and would indeed in multitudes of instances have been out of the question. We have no demonstrative proof that this state of things is universal; but having found it prevail extensively, we go on with the resolution at least to try for it everywhere else, and we are not disappointed. From propositions obtained in this way, or rather from the original facts on which these propositions rest, we can make *new* inferences, not indeed with absolute certainty, but with a degree of conviction that is of the utmost practical use. We have gained the great step of being able to make trustworthy generalizations. We conclude, for instance, not merely that John and Henry die, but that all men die.

§ 5. The above brief investigation contains, it is hoped, a tolerably correct outline of the nature of the Inductive inference, as it presents itself in Material or Scientific Logic. It involves the distinction drawn by Mill, and with which the reader of his *System of Logic* will be familiar, between an inference drawn *according* to a formula and one drawn *from* a formula. We do in reality make our inference from the data afforded by experience directly to the conclusion; it is a mere arrangement of convenience to do so by passing through the generalization. But it is one of such extreme convenience, and one so necessarily forced upon us when we are appealing to our own past experience or to that of others for the grounds of our conclusion, that practically we find it the best plan to divide the process of inference into two parts. The first part is concerned with establishing the generalization; the second (which contains the rules of ordinary logic) determines what conclusions can be drawn from this generalization.

§ 6. We may now see our way to ascertaining the province of Probability and its relation to kindred sciences. Inductive Logic gives rules for discovering such generalizations as those spoken of above, and for testing their correctness. If they are expressed in universal propositions it is the part of ordinary logic to determine what inferences can be made from and by them; if, on the other hand, they are expressed in proportional propositions, that is, propositions of the kind described in our first chapter, they are handed over to Probability. We find, for example, that three infants out of ten die in their first four years. It belongs to Induction to say whether we are justified in generalizing our observation into the assertion, All infants die in that proportion. When such a proposition is obtained, whatever may be the value to be assigned to it, we recognize in it a series of a familiar kind, and it is at once claimed by Probability.

In this latter case the division into two parts, the inductive and the ratiocinative, seems decidedly more than one of convenience; it is indeed imperatively necessary for clearness of thought and cogency of treatment. It is true that in almost every example that can be selected we shall find both of the above elements existing together and combining to determine the degree of our conviction, but when we come to examine them closely it appears to me that the grounds of their cogency, the kind of conviction they produce, and consequently the rules which they give rise to, are so entirely distinct that they cannot possibly be harmonized into a single consistent system.

The opinion therefore according to which certain Inductive formulæ are regarded as composing a portion of Probability, and which finds utterance in the Rule of Succession criticised in our last chapter, cannot, I think, be maintained. It would be more correct to say, as stated above, that Induction is quite distinct from Probability, yet co-operates in almost all its inferences. By Induction we determine, for example, whether, and how far, we can safely generalize the proposition that four men in ten live to be fifty-six; supposing such a proposition to be safely generalized, we hand it over to Probability to say what sort of inferences can be deduced from it.

§ 7. So much then for the opinion which tends to regard pure Induction as a subdivision of Probability. By the majority of philosophical and logical writers a widely different view has of course been entertained. They are mostly disposed to distinguish these sciences very sharply from, not to say to contrast them with, one another; the one being accepted as philosophical or logical, and the other rejected as mathematical. This may without offence be termed the popular prejudice against Probability.

A somewhat different view, however, must be noticed here, which, by a sort of reaction against the latter, seems even to go beyond the former; and which occasionally finds expression in the statement that all inductive reasoning of every kind is merely a matter of Probability. Two examples of this may be given.

Beginning with the oldest authority, there is an often quoted saying by Butler at the commencement of his *Analogy*, that 'probability is the very guide of life'; a saying which seems frequently to be understood to signify that the rules or principles of Probability are thus all-prevalent when we are drawing conclusions in practical life. Judging by the drift of the context, indeed, this seems a fair interpretation of his meaning, in so far of course as there could be said to be any such thing as a science of Probability in those days. Prof. Jevons, in his *Principles of Science* (Vol. I. p. 224), has expressed a somewhat similar view, of course in a way more consistent with the principles of modern science, physical and mathematical. He says, "I conceive that it is impossible even to expound the principles and methods of induction as applied to natural phenomena, in a sound manner, without resting them on the theory of Probability. Perfect knowledge alone can give certainty, and in nature perfect knowledge would be infinite knowledge, which is clearly beyond our capacities. We have, therefore, to content ourselves with partial knowledge,—knowledge mingled with ignorance, producing doubt[1]."

§ 8. There are two senses in which this disposition to merge the two sciences into one may be understood. Using the word Probability in its vague popular signification, nothing more may be intended than to call attention to the fact, that in every case alike our conclusions are nothing more than 'probable,' that is, that they are not, and cannot be, absolutely certain. This must be fully admitted, for of course no one acquainted with the complexity of physical and other evidence would seriously maintain that absolute ideal certainty can be attained in any branch of applied logic. Hypothetical certainty, in abstract science, is possible, but not absolute certainty in the domain of the concrete. This has been already noticed in a former chapter, where, however, it was pointed out that whatever justification may exist, on the subjective view of logic, for regarding this common prevalence of absence of certainty as warranting us in fusing the sciences into one, no such justification is admitted when we take the objective view.

§ 9. What may be meant, however, is that the *grounds* of this absence of certainty are always of the same general character. This argument, if admitted, would have real force, and must therefore be briefly noticed. We have seen abundantly that when we say of a conclusion within the strict province of Probability, that it is not certain, all that we mean is that in some proportion of cases only will such conclusion be right, in the other cases it will be wrong. Now when we say, in reference to any inductive conclusion, that we feel uncertain about its absolute cogency, are we conscious of the same interpretation? It seems to me that we are not. It is indeed quite possible that on ultimate analysis it might be proved that experience of failure in the past employment of our methods of investigation, or expectation of failure in our future employment of them, was the main cause of our present want of perfect confidence in them. But this, as we have repeatedly insisted, does not belong to the province of logical, but to that of Psychological enquiry. It is surely not the case that we are, as a rule, consciously guided by such occasional or repeated instances of past failure. In so far as they are at all influential, they seem to do their work by infusing a vague want of confidence which cannot be referred to any statistical grounds for its justification, at least not in a quantitative way. Part of our want of confidence is derived sympathetically from those who have investigated the matter more nearly at first hand. Here again, analysis might detect that a given proportion of past failures lay at the root of the distrust, but it does not show at the surface. Moreover, one reason why we cannot feel perfectly certain about our inductions is, that the *memory* has to be appealed to for some of our data; and will any one assert that the only reason why we do not place absolute reliance on our memory of events long past is that we have been deceived in that way before?

---

[1] See also Dugald Stewart (Ed. by Hamilton; VII. pp. 115—119).

In any other sense, therefore, than as a needful protest against attaching too great demonstrative force to the conclusions of Inductive Logic, it seems decidedly misleading to speak of its reasonings as resting upon Probability.

§ 10. We may now see clearly the reasons for the limits within which causation[1] is necessarily required, but beyond which it is not needed. To be able to generalize a formula so as to extend it from the observed to the unobserved, it is clearly essential that there should be a certain permanence in the order of nature; this permanence is one form of what is implied in the term causation. If the circumstances under which men live and die remaining the same, we did not feel warranted in inferring that four men out of ten would continue to live to fifty, because in the case of those whom we had observed this proportion had hitherto done so, it is clear that we should be admitting that the same antecedents need not be followed by the same consequents. This uniformity being what the Law of Causation asserts, the truth of the law is clearly necessary to enable us to obtain our generalizations: in other words, it is necessary for the Inductive part of the process. But it seems to be equally clear that causation is not necessary for that part of the process which belongs to Probability. Provided only that the truth of our generalizations is secured to us, in the way just mentioned, what does it matter to us whether or not the individual members are subject to causation? For it is not in reality about these individuals that we make inferences. As this last point has been already fully treated in Chapter VI, any further allusion to it need not be made here.

---

[1] Required that is for purposes of logical inference within the limits of Probability; it is not intended to imply any doubts as to its actual universal prevalence, or its all-importance for scientific purposes. The subject is more fully discussed in a future chapter.

§ 11. The above description, or rather indication, of the process of obtaining these generalizations must suffice for the present. Let us now turn and consider the means by which we are practically to make use of them when they are obtained. The point which we had reached in the course of the investigations entered into in the fifth and sixth chapters was this:—Given a series of a certain kind, we could draw inferences about the members which composed it; inferences, that is, of a peculiar kind, the value and meaning of which were fully discussed in their proper place.

We must now shift our point of view a little; instead of starting, as in the former chapters, with a determinate series supposed to be given to us, let us assume that the individual only is given, and that the work is imposed upon us of finding out the appropriate series. How are we to set about the task? In the former case our data were of this kind:—Eight out of ten men, aged fifty, will live eleven years more, and we ascertained in what sense, and with what certainty, we could infer that, say, John Smith, aged fifty, would live to sixty-one.

§ 12. Let us then suppose, instead, that John Smith presents himself, how should we in this case set about obtaining a series for him? In other words, how should we collect the appropriate statistics? It should be borne in mind that when we are attempting to make real inferences about things as yet unknown, it is in this form that the problem will practically present itself.

At first sight the answer to this question may seem to be obtained by a very simple process, viz. by counting how many men of the age of John Smith, respectively do and do not live for eleven years. In reality however the process is far from being so simple as it appears. For it must be remembered that each individual thing has not one distinct and appropriate series, to which, and to which alone, it properly belongs. We may indeed be practically in the habit of considering it under such a single aspect, and it may therefore seem to us more familiar when it occupies a place in one series rather than in another; but such a practice is merely customary on our part, not obligatory. It is obvious that every individual thing or event has an indefinite number of properties or attributes observable in it, and might therefore be considered as belonging to an indefinite number of different classes of things. By belonging to any one class it of course becomes at the same time a member of all the higher classes, the genera, of which that class was a species. But, moreover, by virtue of each accidental attribute which it possesses, it becomes a member of a class intersecting, so to say, some of the other classes. John Smith is a consumptive man say, and a native of a northern climate. Being a man he is of course included in the class of vertebrates, also in that of animals, as well as in any higher such classes that there may be. The property of being consumptive refers him to another class, narrower than any of the above; whilst that of being born in a northern climate refers him to a new and distinct class, not conterminous with any of the rest, for there are things born in the north which are not men.

§ 13. When therefore John Smith presents himself to our notice without, so to say, any particular label attached to him informing us under which of his various aspects he is to be viewed, the process of thus referring him to a class becomes to a great extent arbitrary. If he had been indicated to us by a general name, that, of course, would have been some clue; for the name having a determinate connotation would specify at any rate a fixed group of attributes within which our selection was to be confined. But names and attributes being connected together, we are here supposed to be just as much in ignorance what name he is to be called by, as what group out of all his innumerable attributes is to be taken account of; for to tell us one of these things would be precisely the same in effect as to tell us the other. In saying that it is thus arbitrary under which class he is placed, we mean, of course, that there are no logical grounds of decision; the selection must be determined by some extraneous considerations. Mere inspection of the individual would simply show us that he could equally be referred to an indefinite number of classes, but would in itself give no inducement to prefer, for our special purpose, one of these classes to another.

This variety of classes to which the individual may be referred owing to his possession of a multiplicity of attributes, has an important bearing on the process of inference which was indicated in the earlier sections of this chapter, and which we must now examine in more special reference to our particular subject.

§ 14. It will serve to bring out more clearly the nature of some of those peculiarities of the step which we are now about to take in the case of Probability, if we first examine the form which the corresponding step assumes in the case of ordinary Logic. Suppose then that we wished to ascertain whether a certain John Smith, a man of thirty, who is amongst other things a resident in India, and distinctly affected with cancer, will continue to survive there for twenty years longer. The terms in which the man is thus introduced to us refer him to different classes in the way already indicated. Corresponding to these classes there will be a number of propositions which have been obtained by previous observations and inductions, and which we may therefore assume to be available and ready at hand when we want to make use of them. Let us conceive them to be such as these following:—Some men live to fifty; some Indian residents live to fifty; no man suffering from cancer lives for five years more. From the first and second of these premises nothing whatever can be inferred, for they are both[1] particular propositions, and therefore lead to no conclusion in this case. The third answers our enquiry decisively.

To the logical reader it will hardly be necessary to point out that the process here under consideration is that of finding middle terms which shall serve to connect the subject and predicate of our conclusion. This subject and predicate in the case in question, are the individual before us and his death within the stated period. Regarded by themselves there is nothing in common between them, and therefore no link by which they may be connected or disconnected with each other. The various classes above referred to are a set of such middle terms, and the propositions belonging to them are a corresponding set of major premises. By the help of any one of them we are enabled, under suitable circumstances, to connect together the subject and predicate of the conclusion, that is, to infer whether the man will or will not live twenty years.

---

[1] As particular propositions they are both of course identical in form. The fact that the 'some' in the former corresponds to a larger proportion than in the latter, is a distinction alien to pure Logic.

§ 15. Now in the performance of such a logical process there are two considerations to which the reader's attention must for a moment be directed. They are simple enough in this case, but will need careful explanation in the corresponding case in Probability. In the first place, it is clear that whenever we can make any inference at all, we can do so with absolute certainty. Logic, within its own domain, knows nothing of hesitation or doubt. If the middle term is appropriate it serves to connect the extremes in such a way as to preclude all uncertainty about the conclusion; if it is not, there is so far an end of the matter: no conclusion can be drawn, and we are therefore left where we were. Assuming our premises to be correct, we either know our conclusion for certain, or we know nothing whatever about it. In the second place, it should be noticed that none of the possible alternatives in the shape of such major premises as those given above can ever contradict any of the others, or be at all inconsistent with them. Regarded as isolated propositions, there is of course nothing to secure such harmony; they have very different predicates, and may seem quite out of each other's reach for either support or opposition. But by means of the other premise they are in each case brought into relation with one another, and the general interests of truth and consistency prevent them therefore from contradicting one another. As isolated propositions it might have been the case that 'all men live to fifty,' and that no Indian residents do so, but having recognised that some men are residents in India, we see at once that these premises are inconsistent, and therefore that one or other of them must be rejected. In all applied logic this necessity of avoiding self-contradiction is so obvious and imperious that no one would think it necessary to lay down the formal postulate that all such possible major premises are to be mutually consistent. To suppose that this postulate is not complied with, would be in effect to make two or more contradictory assumptions about matters of fact.

§ 16. But now observe the difference when we attempt to take the corresponding step in Probability. For ordinary propositions, universal or particular, substitute statistical propositions of what we have been in the habit of calling the 'proportional' kind. In other words, instead of asking whether the man will live for twenty years, let us ask whether he will live for one year? We shall be unable to find any universal propositions which will cover the case, but we may without difficulty obtain an abundance of appropriate proportional ones. They will be of the following description:—Of men aged 30, 98 in 100 live another year; of residents in India a smaller proportion survive, let us for example say 90 in 100; of men suffering from cancer a smaller proportion still, let us say 20 in 100.

Now in both of the respects to which attention has just been drawn, propositions of this kind offer a marked contrast with those last considered. In the first place, they do not, like ordinary propositions, either assert unequivocally yes or no, or else refuse to open their lips; but they give instead a sort of qualified or hesitating answer concerning the individuals included in them. This is of course nothing more than the familiar characteristic of what may be called 'probability propositions.' But it leads up to, and indeed renders possible, the second and more important point; viz. that these various answers, though they cannot directly and formally contradict one another (this their nature as proportional propositions, will not as a rule permit), may yet, in a way which will now have to be pointed out, be found to be more or less in conflict with one another.

Hence it follows that in the attempt to draw a conclusion from premises of the kind in question, we may be placed in a position of some perplexity; but it is a perplexity which may present itself in two forms, a mild and an aggravated form. We will notice them in turn.

§ 17. The mild form occurs when the different classes to which the individual case may be appropriately referred are successively included one within another; for here our sets of statistics, though leading to different results, will not often be found to be very seriously at variance with one another. All that comes of it is that as we ascend in the scale by appealing to higher and higher genera, the statistics grow continually less appropriate to the particular case in point, and such information therefore as they afford becomes gradually less explicit and accurate.

§ 18. The question that we originally wanted to determine, be it remembered, is whether John Smith will die within one year. But all knowledge of this fact being unattainable, owing to the absence of suitable inductions, we felt justified (with the explanation, and under the restrictions mentioned in Chap. V.), in substituting, as the only available equivalent for such individual knowledge, the answer to the following statistical enquiry, What proportion of men in his circumstances die?

But then at once there begins to arise some doubt and ambiguity as to what exactly is to be understood by his circumstances. We may know very well what these circumstances are in themselves, and yet be in perplexity as to how many of them we ought to take into account when endeavouring to estimate his fate. We might conceivably, for a beginning, choose to confine our attention to those properties only which he has in common with all animals. If so, and statistics on the subject were attainable, they would presumably be of some such character as this, Ninety-nine animals out of a hundred die within a year. Unusual as such a reference would be, we should, logically speaking, be doing nothing more than taking a wider class than the one we were accustomed to. Similarly we might, if we pleased, take our stand at the class of vertebrates, or at that of mammalia, if zoologists were able to give us the requisite information. Of course we reject these wide classes and prefer a narrower one. If asked why we reject them, the natural answer is that they are so general, and resemble the particular case before us in so few points, that we should be exceedingly likely to go astray in trusting to them. Though accuracy cannot be insured, we may at least avoid any needless exaggeration of the relative number and magnitude of our errors.

§ 19. The above answer is quite valid; but whilst cautioning us against appealing to too wide a class, it seems to suggest that we cannot go wrong in the opposite direction, that is in taking too narrow a class. And yet we do avoid any such extremes. John Smith is not only an Englishman;

he may also be a native of such a part of England, be living in such a Presidency, and so on. An indefinite number of such additional characteristics might be brought out into notice, many of which at any rate have some bearing upon the question of vitality. Why do we reject any consideration of these narrower classes? We do reject them, but it is for what may be termed a practical rather than a theoretical reason. As was explained in the first chapters, it is essential that our series should contain a considerable number of terms if they are to be of any service to us. Now many of the attributes of any individual are so rare that to take them into account would be at variance with the fundamental assumption of our science, viz. that we are properly concerned only with the averages of large numbers. The more special and minute our statistics the better, provided only that we can get enough of them, and so make up the requisite large number of instances. This is, however, unfortunately impossible in many cases. We are therefore obliged to neglect one attribute after another, and so enlarge the contents of our class; at the avowed risk of somewhat increased variety and unsuitability in the members of it, for at each step of this kind we diverge more and more from the sort of instances that we really want. We continue to do so, until we no longer gain more in quantity than we lose in quality. We finally take our stand at the point where we first obtain statistics drawn from a sufficiently large range of observation to secure the requisite degree of stability and uniformity.

§ 20. In such an example as the one just mentioned, where one of the successive classes—man—is a well-defined natural kind or species, there is such a complete break in each direction at this point, that every one is prompted to take his stand here. On the one hand, no one would ever think of introducing any reference to the higher classes with fewer attributes, such as animal or organized being: and on the other hand, the inferior classes, created by our taking notice of his employment or place of residence, &c., do not as a rule differ sufficiently in their characteristics from the class *man* to make it worth our while to attend to them.

Now and then indeed these characteristics do rise into importance, and whenever this is the case we concentrate our attention upon the class to which they correspond, that is, the class which is marked off by their presence. Thus, for instance, the quality of consumptiveness separates any one off so widely from the majority of his fellow-men in all questions pertaining to mortality, that statistics about the lives of consumptive men differ materially from those which refer to men in general. And we see the result; if a consumptive man can effect an insurance at all, he must do it for a much higher premium, calculated upon his special circumstances. In other words, the attribute is sufficiently important to mark off a fresh class or series. So with Insurance against accident. It is not indeed attempted to make a special rate of insurance for the members of each separate trade, but the differences of risk to which they are liable oblige us to take such facts to some degree into account. Hence, trades are roughly divided into two or three classes, such as the ordinary, the hazardous, and the extra-hazardous, each having to pay its own rate of premium.

§ 21. Where one or other of the classes thus corresponds to natural kinds, or involves distinctions of co-ordinate importance with those of natural kinds, the process is not difficult; there is almost always some one of these classes which is so universally recognized to be the appropriate one, that most persons are quite unaware of there being any necessity for a process of selection. Except in the cases where a man has a sickly constitution, or follows a dangerous employment, we seldom have occasion to collect statistics for him from any class but that of men in general of his age in the country.

When, however, these successive classes are not ready marked out for us by nature, and thence arranged in easily distinguishable groups, the process is more obviously arbitrary. Suppose we were considering the chance of a man's house being burnt down, with what collection of attributes should we rest content in this instance? Should we include all kinds of buildings, or only dwelling-houses, or confine ourselves to those where there is much wood, or those which have stoves? All these attributes, and a multitude of others may be present, and, if so, they are all circumstances which help to modify our judgment. We must be guided here by the statistics which we happen to be able to obtain in sufficient numbers. Here again, rough distinctions of this kind are practically drawn in Insurance Offices, by dividing risks into ordinary, hazardous, and extra-hazardous. We examine our case, refer it to one or other of these classes, and then form our judgment upon its prospects by the statistics appropriate to its class.

So much for what may be called the mild form in which the ambiguity occurs; but there is an aggravated form in which it may show itself, and which at first sight seems to place us in far greater perplexity.

§ 22. Suppose that the different classes mentioned above are not included successively one within the other. We may then be quite at a loss which of the statistical tables to employ. Let us assume, for example, that nine out of ten Englishmen are injured by residence in Madeira, but that nine out of ten consumptive persons are benefited by such a residence. These statistics, though fanciful, are conceivable and perfectly compatible[1]. John Smith is a consumptive Englishman; are we to recommend a visit to Madeira in his case or not? In other words, what inferences are we to draw about the probability of his death? Both of the statistical tables apply to his case, but they would lead us to directly contradictory conclusions. This does not mean, of course, contradictory precisely in the logical sense of that word, for one of these propositions does not assert that an event must happen and the other deny that it must; but contradictory in the sense that one would cause us in some considerable degree to believe what the other would cause us in some considerable degree to disbelieve. This refers, of course, to the individual events; the statistics are by supposition in no degree contradictory. Without further data, therefore, we can come to no decision.

§ 23. Practically, of course, if we were forced to a decision with only these data before us, we should make our choice by the consideration that the state of a man's lungs has probably more to do with his health than the place of his birth has; that is, we should conclude that the duration of life of consumptive Englishmen corresponds much more closely with that of consumptive persons in general than with that of their healthy countrymen. But this is, of course, to import alien considerations into the question. The data, as they are given to us, and if we confine ourselves to them, leave us in absolute uncertainty upon the point. It may be that the consumptive Englishmen almost all die when transported into the other climate; it may be that they almost all recover. If they die, this is in obvious accordance with the first set of statistics; it will be found in accordance with the second set through the fact of the foreign consumptives profiting by the change of climate in more than what might be termed their due proportion. A similar explanation will apply to the other alternative, viz. to the supposition that the consumptive Englishmen mostly recover. The problem is, therefore, left absolutely indeterminate, for we cannot here appeal to any general rule so simple and so obviously applicable as that which, in a former case, recommended us always to prefer the more special statistics, when sufficiently extensive, to those which are wider and more general. We have no means here of knowing whether one set is more special than the other.

---

[1] I have mostly attempted to give examples of life statistics that shall be correct; but when they have to be invented this may as well be done completely, at the risk (as I believe is the case here) of flagrant violation of facts. The peculiarity intended to be illustrated above does undoubtedly present itself in many directions, but I cannot discover any example that shall both exhibit it to a sufficiently marked extent, and with due numerical determinateness, to serve as a convenient logical example.

And in this no difficulty can be found, so long as we confine ourselves to a just view of the subject. Let me again recall to the reader's mind what our present position is; we have substituted for knowledge of the individual (finding that unattainable) a knowledge of what occurs in the average of similar cases. This step had to be taken the moment the problem was handed over to Probability. But the conception of similarity in the cases introduces us to a perplexity; we manage indeed to evade it in many instances, but here it is inevitably forced upon our notice. There are here two aspects of this similarity, and they introduce us to two distinct averages. Two assertions are made as to what happens in the long run, and both of these assertions, by supposition, are verified. Of their truth there need be no doubt, for both were supposed to be obtained from experience.

§ 24. It may perhaps be supposed that such an example as this is a *reductio ad absurdum* of the principle upon which Life and other Insurances are founded. But a moment's consideration will show that this is quite a mistake, and that the principle of Assurance is just as applicable to examples of this kind as to any other. An Office need find no difficulty in the case supposed. They *might* (for a reason to be mentioned presently, they probably *would* not) insure the individual without inconsistency at a rate determined by either average. They might say to him, "You are an Englishman. Out of the multitude of English who come to us nine in ten die if they go to Madeira. We will insure you at a rate assigned by these statistics, knowing that in the long run all will come right so far as we are concerned. You are also consumptive, it is true, and we do not know what proportion of the English are consumptive, nor what proportion of English consumptives die in Madeira. But this does not really matter for our purpose. The formula, nine in ten die, is in reality calculated by taking into account these unknown proportions; for, though we do not know them in themselves, statistics tell us all that we care to know about their results. In other words, whatever unknown elements may exist, must, in regard to all the effects which they can produce, have been already taken into account, so that our ignorance about them cannot in the least degree invalidate such conclusions as we are able to draw. And this is sufficient for our purpose." But precisely the same language might be held to him if he presented himself as a consumptive man; that is to say, the Office could safely carry on its proceedings upon either alternative.

This would, of course, be a very imperfect state for the matter to be left in. The only rational plan would be to isolate the case of consumptive Englishmen, so as to make a separate calculation for their circumstances. This calculation would then at once supersede all other tables so far as they were concerned; for though, *in the end,* it could not arrogate to itself any superiority over the others, it would in the mean time be marked by fewer and slighter aberrations from the truth.

§ 25. The real reason why the Insurance Office could not long work on the above terms is of a very different kind from that which some readers might contemplate, and belongs to a class of considerations which have been much neglected in the attempts to construct sciences of the different branches of human conduct. It is nothing else than that annoying contingency to which prophets since the time of Jonah have been subject, of uttering *suicidal* prophecies; of publishing conclusions which are perfectly certain when every condition and cause but one have been taken into account, that one being the effect of the prophecy itself upon those to whom it refers.

In our example above, the Office (in so far as the particular cases in Madeira are concerned) would get on very well until the consumptive Englishmen in question found out what much better terms they could make by announcing themselves as consumptives, and paying the premium appropriate to that class, instead of announcing themselves as Englishmen. But if they did this they would of course be disturbing the statistics. The tables were based upon the assumption that a certain fixed proportion (it does not matter what proportion) of the English lives would continue to be consumptive lives, which, under the supposed circumstances, would probably soon cease to be true. When it is said that nine Englishmen out of ten die in Madeira, it is meant that of those who come to the Office, as the phrase is, at random, or in their fair proportions, nine-tenths die. The consumptives are supposed to go there just like red-haired men, or poets, or any other special class. Or they might go in any proportions greater or less than those of other classes, so long as they adhered to the same proportion throughout. The tables are then calculated on the continuance of this state of things; the practical contradiction is in supposing such a state of things to continue after the people had once had a look at the tables. If we merely make the assumption that the publication of these tables made no such alteration in the conduct of those to whom it referred, no hitch of this kind need occur.

§ 26. The assumptions here made, as has been said, are not in any way contradictory, but they need some explanation. It will readily be seen that, taken together, they are inconsistent with the supposition that each of these classes is homogeneous, that is, that the statistical proportions which hold of the whole of either of them will also hold of any portion of them which we may take. There are certain individuals (viz. the consumptive Englishmen) who belong to each class, and of course the two different sets of statistics cannot both be true of them taken by themselves. They might coincide in their characteristics with either class, but not with both; probably in most practical cases they will coincide with neither, but be of a somewhat intermediate character. Now when it is said of any such heterogeneous body that, say, nine-tenths die, what is meant (or rather implied) is that the class might be broken up into smaller subdivisions of a more homogeneous character, in some of which, of course, more than nine-tenths die, whilst in others less, the differences depending upon their character, constitution, profession, &c.; the number of such divisions and the amount of their divergence from one another being perhaps very considerable.

Now when we speak of either class as a whole and say that nine-tenths die, the most natural and soundest meaning is that that would be the proportion if all without exception went abroad, or (what comes to the same thing) if each of these various subdivisions was represented in fair proportion to its numbers. Or it might only be meant that they go in some other proportion, depending upon their tastes, pursuits, and so on. But whatever meaning be adopted one condition is necessary, viz. that the proportion of each class that went at the time the statistics were drawn up must be adhered to throughout. When the class is homogeneous this is not needed, but when it is heterogeneous the statistics would be interfered with unless this condition were secured.

We are here supposed to have two sets of statistics, one for the English and one for the consumptives, so that the consumptive English are in a sense counted twice over. If their mortality is of an intermediate amount, therefore, they serve to keep down the mortality of one class and to keep up that of the other. If the statistics are supposed to be exhaustive, by referring to the whole of each class, it follows that actually the same individuals must be counted each time; but if representatives only of each class are taken, the same individuals need not be inserted in each set of tables.

§ 27. When therefore they come to insure (our remarks are still confined to our supposed Madeira case), we have some English consumptives counted as English, and paying the high rate; and others counted as consumptives and paying the low rate. Logically indeed we may suppose them all entered in each class, and paying therefore each rate. What we have said above is that any individual may be conceived to present himself for either of these classes. Conceive that some one else pays his premium for him, so that it is a matter of indifference to him personally at which rate he insures, and there is nothing to prevent some of the class (or for that matter all) going to one class, and others (or all again) going to the other class.

So long therefore as we make the logically possible though practically absurd supposition that some men will continue to pay a higher rate than they need, there is nothing to prevent the English consumptives (some or all) from insuring in each category and paying its appropriate premium. As soon as they gave any thought to the matter, of course they would, in the case supposed, all prefer to insure as consumptives. But their doing this would disturb each set of statistics. The English mortality in Madeira would instantly become heavier, so far as the Insurance company was concerned, by the loss of all their best lives; whilst the consumptive statistics (unless *all* the English consumptives had already been taken for insurance) would be in the same way deteriorated[1]. A slight readjustment therefore of each scale of insurance would then be needed; this is the disturbance mentioned just above. It must be clearly understood, however, that it is not our original statistics which have proved to be inconsistent, but simply that there were practical obstacles to carrying out a system of insurance upon them.

§ 28. Examples subject to the difficulty now under consideration will doubtless seem perplexing to the student unacquainted with the subject. They are difficult to reconcile with any other view of the science than that insisted on throughout this Essay, viz. that we are only concerned with averages. It will perhaps be urged that there are two different values of the man's life in these cases, and that they cannot both be true. Why not? The 'value' of his life is simply the number of years to which men in his circumstances do, on the average, attain; we have the man set before us under two different circumstances; what wonder, therefore, that these should offer different averages? In such an objection it is forgotten that we have had to substitute for the unattainable result about the individual, the really attainable result about a set of men as much like him as possible. The difficulty and apparent contradiction only arise when people will try to find some justification for their belief in the individual case. What can we possibly conclude, it may be asked, about this particular man John Smith's prospects when we are thus offered two different values for his life? Nothing whatever, it must be replied; nor could we in reality draw a conclusion, be it remembered, in the former case, when we were practically confined to one set of statistics. There also we had what we called the 'value' of his life, and since we only knew of one such value, we came to regard it as in some sense appropriate to him as an individual. Here, on the other hand, we have two values, belonging to different series, and as these values are really different it may be complained that they are discordant, but such a complaint can only be made when we do what we have no right to do, viz. assign a value to the individual which shall admit of individual justification.

---

[1] The reason is obvious. The healthiest English lives in Madeira (viz. the consumptive ones) have now ceased to be reckoned as English; whereas the worst consumptive lives there (viz. the English) are now increased in relative numbers.

§ 29. Is it then perfectly arbitrary what series or class of instances we select by which to judge? By no means; it has been stated repeatedly that in choosing a series, we must seek for one the members of which shall resemble our individual in as many of his attributes as possible, subject only to the restriction that it must be a sufficiently extensive series. What is meant is, that in the above case, where we have two series, we cannot fairly call them contradictory; the only valid charge is one of incompleteness or insufficiency for their purpose, a charge which applies in exactly the same sense, be it remembered, to all statistics which comprise genera unnecessarily wider than the species with which we are concerned. The only difference between the two different classes of cases is, that in the one instance we are on a path which we know will lead at the last, through many errors, towards the truth (in the sense in which truth can be attained here), and we took it for want of a better. In the other instance we have two such paths, perfectly different paths, either of which however will lead us towards the truth as before. Contradiction can only seem to arise when it is attempted to justify each separate step on our paths, as well as their ultimate tendency.

Still it cannot be denied that these objections are a serious drawback to the completeness and validity of any anticipations which are merely founded upon statistical frequency, at any rate in an early stage of experience, when but few statistics have been collected. Such knowledge as Probability can give is not in any individual case of a high order, being subject to the characteristic infirmity of repeated error; but even when measured by its own standard it commences at a very low stage of proficiency. The errors are then relatively very numerous and large compared with what they may ultimately be reduced to.

§ 30. Here as elsewhere there is a continuous process of specialization going on. The needs of a gradually widening experience are perpetually calling upon us to subdivide classes which are found to be too heterogeneous. Sometimes the only complaint that has to be made is that the class to which we are obliged to refer is found to be somewhat too broad to suit our purpose, and that it might be subdivided with convenience. This is the case, as has been shown above, when an Insurance office finds that its increasing business makes it possible and desirable to separate off the men who follow some particular trades from the rest of their fellow-countrymen. Similarly in every other department in which statistics are made use of. This increased demand for specificness leads, in fact, as naturally in this direction, as does the progress of civilization to the subdivision of trades in any town or country. So in reference to the other kind of perplexity mentioned above. Nothing is more common in those sciences or practical arts, in which deduction is but little available, and where in consequence our knowledge is for the most part of the empirical kind, than to meet with suggestions which point more or less directly in contrary directions. Whenever some new substance is discovered or brought into more general use, those who have to deal with it must be familiar with such a state of things. The medical man who has to employ a new drug may often find himself confronted by the two distinct recommendations, that on the one hand it should be employed for certain diseases, and that on the other hand it should not be tried on certain constitutions. A man with such a constitution, but suffering from such a disease, presents himself; which recommendation is the doctor to follow? He feels at once obliged to set to work to collect narrower and more special statistics, in order to escape from such an ambiguity.

§ 31. In this and a multitude of analogous cases afforded by the more practical arts it is not of course necessary that numerical data should be quoted and appealed to; it is sufficient that the judgment is more or less consciously determined by them. All that is necessary to make the examples appropriate is that we should admit that in their case statistical data are our ultimate appeal in the present state of knowledge. Of course if the empirical laws can be resolved into their component causes we may appeal to direct deduction, and in this case the employment of statistics, and consequently the use of the theory of Probability, may be superseded.

In this direction therefore, as time proceeds, the advance of statistical refinement by the incessant subdivision of classes to meet the developing wants of man is plain enough. But if we glance backwards to a more primitive stage, we shall soon see in what a very imperfect state the operation commences. At this early stage, however, Probability and Induction are so closely connected together as to be very apt to be merged into one, or at any rate to have their functions confounded. It will be necessary therefore to examine their early stages more minutely, and with more direct reference to Induction. Before doing so, the results obtained up to this point may be summarized as follows:—

§ 32. Since the generalization of our statistics is found to belong to Induction, this process of generalization may be regarded as prior to, or at least independent of, Probability. We have, moreover, already discussed (in Chapter V.) the step corresponding to what are termed immediate inferences, and (in Chapter VI.) that corresponding to syllogistic inferences. Our present position therefore is that in which we may consider ourselves in possession of any number of generalizations, but wish to employ them so as to make inferences about a given individual; just as in one department of common logic we are engaged in finding middle terms to establish the desired conclusion. In this latter case the process is found to be extremely simple, no accumulation of different middle terms being able to lead to any real ambiguity or contradiction. In Probability, however, the case is different. Here, if we attempt to draw inferences about the individual case before us, as often is attempted—in the Rule of Succession for example—we shall encounter the full force of this ambiguity and contradiction. Treat the question, however, fairly, and all difficulty disappears. Our inference really is not about the individuals as individuals, but about series or successions of them. We wished to know whether John Smith will die within the year; this, however, cannot be known. But John Smith, by the possession of many attributes, belongs to many different series. The multiplicity of middle terms, therefore, is what ought to be expected. We *can* know whether a succession of men, residents in India, consumptives, &c. die within a year. We may make our selection, therefore, amongst these, and in the long run the belief and consequent conduct of ourselves and other persons (as described in Chapter V.) will become capable of justification. With regard to choosing one of these series rather than another, we have two opposing principles of guidance. On the one hand, the more special the series the better; for, though not more right in the end, we shall thus be more nearly right all along. But, on the other hand, if we try to make the series too special, we shall generally meet the practical objection arising from insufficient statistics.

§ 33. Throughout the discussions in this chapter it has been assumed that the common property which was observed in different individuals, and was then by the Inductive act generalized throughout a definite or indefinite class, is one about the existence and nature of which there could never be any doubt or dispute. It was taken for granted that every one would not only recognize it when it was pointed out to him, but that he was also familiar with it already. Most of the examples commonly given, and the discussions to which they often lead, tend very much to confirm such an opinion.

All examination of this point has been deferred up to the present moment, in order not to break the line of enquiry; but it will be advisable now to devote some pages to ascertain how far the assumption spoken of above can be justified. The enquiry is, indeed, only indirectly connected with Probability, at least on the view of that science entertained in this Essay, but at the same time the connection though indirect is very close. Induction is involved in almost every example in Probability, more especially in the early stages of each; and the nature of the relation between these sciences has been a source of so much error and confusion, that it has been found quite impossible to state accurately the opinion here adopted about the latter science without making constant inroads into the former. It is the more necessary to state this opinion fully, in consequence of the great authority of J. S. Mill being apparently in support of what I cannot but regard as an imperfect view of the subject.

§ 34. It will be necessary to commence with a definition of Induction. Let us start with that of J. S. Mill, which, though given by him in various forms, will be expressed with substantial accuracy as follows:—Induction is that act of the mind by which, from observation of a certain definite number of things, we make an inference extending to an indefinite number of them. In this definition, which is in accordance with the investigation in the earlier part of this chapter, it is assumed that the data, viz. the limited number of things from which the formula starts, and on which it is grounded, are already clearly recognized. By this is meant that we are more or less familiar with them as a class, that they have been already grouped together in the mind, and the association fixed by the help of a name. In every example, indeed, such recognition is almost necessarily presupposed before the example can be stated. Whether we take the simple symbolic one, this A and that A, and so on, are X; therefore every A is X; or any special concrete instance that the Inductive Sciences may offer, all practical difficulty which may have existed as to discovering and recognizing our A is omitted from view. But though this omission is possible when we are only offering examples, it is scarcely possible when we are really engaged in making original inferences. The objects from which our inference started as its basis must have been selected; and since this selection was neither made at random nor performed for us by others, there must have been some principle of selection in our minds. The selection, on the first occasion on which it had to be made, may sometimes appear little more than a guess, but even then it is the sagacious guess of one whose mind has been disciplined to the work.

§ 35. As most readers of these pages will know, Whewell has strongly insisted on this selective or, as it may almost be termed, *creative* part of the process of Induction, and has applied a particular technical expression (the introduction of the conception) to denote it[1]. Being probably one of the few who have deduced their philosophical schemes from a careful historical investigation of the processes by which science has been actually constructed, he has not unnaturally attached extreme importance to this part of the act of Induction, and has incorporated it into his definition of Induction. He sometimes uses rather strong expressions to describe it, but probably what has just been mentioned is all that is meant when he speaks of the element which is introduced by the mind, and is not found in the things. The data for the Induction, therefore, in all original examples, have to be selected by a process which the common examples, mostly drawn from instances already familiar to us, tend to let drop out of sight.

---

[1] "Thus in each inference made by Induction, there is introduced some General Conception, which is given, not by the phenomena, but by the mind" (*Nov. Org. Renov.* p. 73).

Let us take one of these familiar examples for closer examination; for instance, the favourite one:—This man is mortal, that man is mortal, and so on; therefore all men are mortal. Now here it is obvious that the 'conception,' to use Whewell's term, is one with which we are already familiar. The collection of attributes which make up what we understand in the abstract by humanity has been constantly united in the mind; the group of objects denoted by the name 'man' has been constantly thought of as a whole; and this association has been powerfully aided by the influence of common language. Nobody, therefore, can see one of the objects which contain this collection of attributes without having the class recalled to him, or at least without having some of the individual objects which compose the class clearly separated off and brought before his mind. And similarly with regard to the attribute of mortality; we are well acquainted with the nature and limits of its connotation and denotation. The words 'man' and 'mortal,' therefore, suggest to every mind the appropriate conceptions. Now though, as we have already seen, and as will be noticed again soon, there is still room even here for some ambiguity, this familiarity with the conceptions enormously diminishes the difficulty of making inferences. It completely alters the character of that process, in fact; so that instead of being like a drive to the right point over the open plain, where there is nothing to suggest our taking one line rather than another, it now resembles the choice of one out of a limited number of ruts which have been well worn and marked out by previous passers-by.

§ 36. A slight modification of the example will make it wear a very different aspect. Let us try to find a case in which the conception has not already become familiar to us, and is not marked out already by being linked to a word, and we shall see what a very different relative importance is then assumed by the two parts of the Inductive process. There is some difficulty, it must be observed, in finding an example of the kind required, for unless there be a word already applied to the objects which we group together, the example will become very awkward to state. But we may obviate the difficulty as follows. Let us take an example in which we, having the word, are already familiar with the conception, but suppose the inference to be made by people who are not in possession of any such word; the person who makes the inference will then labour under the difficulty mentioned, whilst we who stand by and criticise him will escape it.

Let us take, then, the following example. This consumptive man, and that consumptive man, and so on, are short-lived; therefore, generally, all consumptive persons are short-lived. To us at the present time this example appears as simple and intelligible as any other example about mortality, but suppose the inference to be made for the first time by some one amongst a people where consumption had not yet been sufficiently known to be marked out and named. The process will then, *to them*, assume a very different form. The cough, the shortness of breathing, &c., which the word at once suggests to us, will have to be singled out and distinguished from a multitude of somewhat similar phenomena. There is room here for an almost infinite amount of observation and experiment. But now suppose that any one had got to this point, and, what is more, had connected the qualities of consumption and short-life in two or three instances. By this time he has gone partly through the process of Induction according to Whewell, but he has only reached its threshold according to Mill. But what more is there left for him to infer? Surely every one who had got to this point would go on to infer, 'therefore all consumptive persons die soon.' Whether they would be right or wrong in so doing is of course immaterial to our present purpose. The inference would be made, and therefore what Mill considers to be Induction would take place so simply and certainly as to be performed almost unconsciously.

§ 37. But there is still another source of doubt and confusion. It needs but a slight observation to perceive that even in the former example, that of man's mortality, the inference is not so simple as it is made to appear. With all the immense help of the previous inductions and observations of others, which are fixed and perpetuated for us in language, there is still a degree of arbitrariness in the process as it is given in common examples. If we try to place ourselves in the position of one making the inference for the first time, we shall see that we might then be involved in serious perplexity[1]. It should be borne in mind that when we state the grounds of the inference in the form, 'this man is mortal, and that man is,' we are presupposing that the observer has already not merely distinguished the class 'man,' but distinguished it as appropriate to his immediate purpose; whereas the grounds of his inference were in reality merely certain *objects*. It is true that these objects belong to the class *man*, but, as I have pointed cut, they belong also to an indefinite number of other classes as well;

---

[1] The question is partly one of definition; that is, of the limitations to be put upon the province of logic. Mill maintains that logic merely *judges* of evidence when offered; but it may be questioned whether the importance assigned to his four "Methods of Experimental Inquiry" is consistent with this limitation.

to classes within that of man; for example, English, European, &c.: to classes without that of man; for example, mammalia, animal, organized being. As it happens, the observer would have been right in his inference whichever of these classes and corresponding conceptions he might have selected; but such considerations show us that there was scope for great ingenuity and for considerable effort of mind in a preliminary process, which, according to Mill, is no part of Induction.

When, therefore, it is said that the mortality of John, Thomas, &c. is the *whole* evidence for the death of Socrates, and for that of men in general, we are, as it appears to me, very much underrating the complexity of the problem. These objects belonged, no doubt, to the class *man*, but they belonged to an indefinite number of other classes also. What made us draw the line just where we did? Why did we not go farther? Is not our belief in the mortality of man strengthened by the death of animals? Is it not influenced by that of organized beings generally? The moment we admit the question to be so indefinite as this, we see that so far from the arbitrary selection of instances, which was given to us, being the whole evidence, it is but a fragment of the evidence. Analogies of every conceivable amount of strength press in upon us from every side, and help to swell or diminish the degree of our belief in the final result.

§ 38. The bearing of all this upon rules of discovery is obvious. In the precise form in which such rules are commonly stated in Probability, it is supposed that when an event has been observed to happen in a certain way, or an object to possess a certain attribute, a given number of times in succession, we are able to assign the degree of belief which should be entertained as to the recurrence of this event or property. The rule, in this particular form, was fully examined and shown to be of little or no use, but we are now able to perceive that independently of its falsity any rule of the kind is impracticable. It is not merely that we are forbidden to say that, because three A's have been found to be X it is therefore 4 to 1 that the next A will be X. Before the problem could be *stated* even, a process that is often very difficult and tedious has to be gone through. A and X have to be recognized and distinguished, and if the conception is one with which we are not familiar this distinction will be very partial and progressive. And when the conception is formed, we should generally find it impossible to limit our grounds of belief to the objects denoted by this conception; an indefinite number of other conceptions would all seem to have an almost equal claim to acceptance. It would be quite unwarrantable to reject entirely all these other conceptions, and if we do not reject them the data on which our belief is founded become almost infinite in number, and therefore vague in value. Things and qualities, it must be remembered, are far from being so sharply discriminated from one another as are letters and symbols. We have observed consumptiveness in Englishmen; is it quite the same quality in people of other countries? Is there not something resembling it in some animals? We may give the quality the same name, but in doing so we must remember that it possesses every conceivable amount of variation in degree; and if we are reasoning with any accuracy this variation should influence the amount of our assent. Moreover, in addition to the indefiniteness caused by the object being thus included in a number of classes which point with varying degrees of force towards the same inferences, there is (as already pointed out) the embarrassment caused by its being included in classes which point towards conflicting inferences.

The above remarks are intended mainly to apply to the case of such induction as it is attempted to introduce into Probability, in which not merely the fact of our belief is taken into account, but also its amount is to be weighed. In such cases the multiplicity and vagueness of the influencing conditions are an insurmountable obstacle in our way. But they also have considerable force in reference to ordinary induction, so as to render it exceedingly doubtful how far such Rules of Anticipation can ever be more than a collection of hints and suggestions for making judicious guesses. When any one has obtained the conception of consumption, and has observed that it is accompanied in certain known cases by short-life, he may and probably will go on to the inference that it will be so in more cases, if not in all. The proposition, therefore, 'All consumptive persons die early', is gradually borne in upon him. He cannot point to any limited, definite number of instances on which this Induction rests; it is supported rather by an indefinite number of analogies more or less close or remote. But the first point from which anything like such accurate reasoning as is capable of being fully expressed in logical forms can commence, is the point at which such propositions are obtained. When such propositions are obtained we are then in a position to draw certain inferences by strict rules. If they are universal propositions they belong to ordinary Logic; if proportional, to Probability.

CHAPTER IX.

CAUSATION AND DESIGN.

§ 1. To understand what a thing *is*, one must generally give some attention to appreciating what it is *not*, especially where the main difficulty of the task is found in the process of explicating a somewhat unfamiliar conception. Applying this plan to the term 'chance', there will be found to be two sets of terms which are very commonly used to indicate an antithesis. One of these is 'causation' and its synonymes, and the other 'choice' or 'design'. These two sets of terms mark in strictness, at any rate as they will be understood here, a very different kind of opposition; but the controversies to which they give rise will be found to overlap, and in some instances to merge into, one another.

We will begin with Causation, which of course comes into perpetual antithesis with such expressions as casual and random. The term, it need not be remarked, is one of many meanings, so it will save trouble to state clearly at the outset that we are not going to enter into any metaphysical discussions. No enquiries will be made as to the intimate nature of the causal connexion, if indeed it have one of which the human faculties can take account, nor need any of the associations excited by the term 'efficient cause' be introduced. Some of these questions will have to be touched upon further on, when we come to speak of choice and design, but at present we shall employ the word *cause* simply in the sense which is becoming very generally accepted by scientific men, viz. invariable unconditional sequence. This acceptation, in fact, is so general that we shall have little else to do here than to suggest a few needful corrections and amplifications of the customary account of the matter.

§ 2. A full exposition of this view will be found in Mill's *Logic.* He points out, that by the 'antecedent' must be understood the sum total of antecedents:—"It is seldom, if ever, between a consequent and a single antecedent that this invariable sequence subsists. It is usually between a consequent and the sum of several antecedents; the concurrence of all of them being necessary to produce, that is, to be certain of being followed by, the consequent. In such cases it is very common to single out one only of the antecedents under the denomination of cause, calling the others merely conditions." Several pages of explanation follow, devoted to tracing out the arbitrary and unphilosophical nature of any such distinction as that mentioned in the last clause, and to enforcing the fact that throughout his work he means to understand by cause "the sum total of the conditions positive and negative, taken together; the whole of the contingencies of every description, which being realized, the consequent invariably follows." (*Logic,* Bk. III. ch. V. § 3.)

We may fairly say that this view of causation is very generally accepted in Science and in the logical treatises on Inductive Philosophy, if indeed it may not be termed the popular view. There are, of course, very wide differences of opinion as to the extent over which it ought to be admitted that causation in this sense prevails; some contending that no phenomena whatever are exempt from it, others claiming that human volitions are an exception. This diversity of opinion, however, as to its range need not affect our hopes of coming to perfect agreement as to its signification. In logical language, having fixed the connotation of the word, it must be left to the reader to assign the denotation according as his philosophical principles demand.

No objections can be raised against the correctness (in a certain sense) of the above view of causation, nor against its usefulness for the logic of direct Induction and of common life. But it seems, nevertheless, to give an inadequate idea of the complexity of the uniformity of nature, and in strictness to admit only of a perfectly accurate enunciation when couched in a hypothetical form. The purport of these remarks will, however, be better seen after we have examined somewhat more in detail one or two characteristics in the definition as thus given.

§ 3. The two principal points to be attended to are,

(1) The introduction of the *sum total* of the antecedents into the cause, (2) the regarding the cause as the *immediate* antecedent. The latter condition is often not so explicitly stated, but we shall easily see that it is implicitly involved.

(1) The cause is defined to be the sum total of the antecedents. But the complexity and variety of nature are far too great to permit us to carry out this summation completely and rigidly; accordingly we have to omit some of the elements which conjointly form the invariable antecedent. It will be easily seen, in fact, that if our definition is to be rigidly accurate, it can hardly be expressed in any but a hypothetical form. Suppose we insist upon accuracy; we then have to say that the cause is such a collection of antecedents as *if* it were repeated the consequent would again follow. But will it be repeated? very seldom, perhaps never, if we insist upon having all the antecedents accurately introduced. In very simple examples we do of course find repetition, or

something undistinguishably resembling it; but the moment we examine cases of any degree of complexity, though there may be repetition of many of the separate elements, the precise combination is generally unique. This would be the case, for instance, with almost everything which affected our bodily or mental constitutions, for no man really is found exactly to resemble another in these respects. So again, if we were examining any such operations of nature as thunderstorms and earthquakes; and even more so in the case of such extensive groups of phenomena as those which compose what we call successive geological epochs.

In such cases as those just mentioned it would be true, speaking roughly, and in almost all cases true, speaking precisely, that the same antecedents never do recur. Definitions, therefore, which presuppose such recurrence (as those clearly do which speak of invariable antecedence), become hypothetical, and really resemble the definitions of such an abstract science as geometry.

§ 4. (2) The other point, viz. the necessity of the sequence being *immediate*, next deserves some notice. The same general remark may be made here,—that we seem reduced to the alternative of sacrificing either the accuracy of our definition or its applicability.

It is clear that when we speak of antecedent and consequent, and of their invariable connexion, we must in strictness assume them to be, so to say, in contact with one another; not separated by any appreciable interval of time. If we allow any interval between them, we lose all security against the intrusion of counteracting agencies, and even against the fluctuations and changes to which the antecedents themselves may be exposed. But then, what *is* an immediate antecedent? Take two points as near to one another as we please, another can always be interposed

[illegible]

[illegible] the familiar [illegible] and a different [illegible] When we hear [illegible] following one another in succession [illegible] one another in nature is [illegible] language. What we really find in nature [illegible] different, being a process of evolution rather than one of succession. The stages which [illegible] from one another are seen to be [illegible], but when closely examined each merges into the next by insensible degrees. They are like the strands of a rope, succeeding one another without break of continuity, so that instead of finding links definitely marked out one from another for us, the steps and stages have to be assigned by ourselves, and have a great deal of what is arbitrary in them.

The same misconception is aided by the unavoidable practice of using letters or other symbols to denote the causes and effects. We speak of the invariable sequence of A and B, or of there being a possible intermediate link C to be detected between them, and so on. Necessary as such language may be if we wish to justify and explain our experiments and inductive reasonings, we have to be on our guard that we are not misled by it. What nature presents is not simply *A* and *B* as successive links, with a possible interpolation of *C*, but rather *A* and *B* as successive portions of a strand, between which if we had chosen we might have interpolated an indefinite number more.

§ 6. The fact is, I think, that this way of regarding the cause springs out of the needs and wishes of practical life, social and scientific, and, in spite of the improvements which the philosopher has effected, it still bears the impress of its origin. The popular way of regarding the order of nature is naturally and rightly that of seeing in it a series of events, sufficiently clearly marked off from one another to be named and readily distinguished. A man is found dead; we want to know whether he was murdered or died of a fit. A glass unexpectedly cracks; was it the sudden pouring in of hot water that did the mischief? In all such cases the antecedents that we have in view consist of a group of proximate phenomena; the philosopher with his microscopic glance may show that at every instant changes were proceeding which led up to further changes, and that there is no portion of time, however minute, which could not be subdivided and shown to display its antecedents and consequents. But this way of looking at it is of no service for common speculative purposes; and for practical purposes, that is, for acquiring the power of producing or averting events, may often be distinctly less convenient.

§ 7. A strong confirmation of this popular origin will be found in the nature of a distinction recognized between the cause and the effect. On the purely phenomenalist view, in which all that is 'efficient' in the cause is rejected, and nothing left but bare sequences, it is not easy to see how there should be such a distinction between them as that an effect may be brought about by several causes, but that given the cause the effect is at once fixed. In other words how is it that we hear of Plurality of causes, but never of Plurality of effects? The immediate reason indeed is not far to seek. It simply consists in this, that we are more exacting in our requirements when we define the cause, than when we define the effect. In the former we insist upon introducing *all* the antecedents, whilst in the latter we are satisfied with a limited number, it may be one or more, of the consequents. If all the antecedents and all the consequents were introduced, either of these groups would equally and absolutely determine the other. From a complete knowledge of the one, the other could infallibly be inferred. For logical purposes, the past is just as much contained in the future as the future in the past[1].

But it is clear that when we are discussing an effect we do not generally, if ever, take into account more than a small selection out of the total group of consequents. In the case of the man's death, for instance, we say that this might have been brought about either by a fit, or by violence, or by suicide, or in some other way. But it is obvious that in so saying we are regarding the mere fact of death only, and omitting all the additional details which serve to characterize it. We know that if we take account of all the consequents, that is, if we insist on introducing all the attendant circumstances, we shall so specialize the case that only one mode of death could possibly have been the cause.

§ 8. What is the explanation of this? The whole ground of this broad distinction between the completeness with which we define the cause and the effect seems to me to arise simply out of the fact that the former goes before the latter. We live, so to say, forwards and not backwards in time. Prevision of consequences is an imperious necessity of life; but the only way of securing what we like and avoiding what we dislike, is by knowing for certain all the antecedents, for unless we do know them all, we may be omitting some vitally important element. There are various ways of obtaining a fire, but to know how to do it in any given case, we must carefully note all the antecedents; this is the sort of knowledge which stands first in practical importance. But to know, in any given case, how a fire *has* been lighted, we should have to take account of all the consequents; this on the other hand is a sort of knowledge, which though often of the highest value, is comparatively of a speculative rather than a practical kind. It therefore comes later, and is less likely to colour our popular way of thinking and expressing ourselves. To a purely speculative race, who took no more interest in the future than in the past, it is not easy to see how any such distinction as this could exist. If they were equally desirous of knowing how things had come to pass, as of knowing how they were to be produced; if, that is, they were as much in the habit of stepping backwards into past time, and determining not merely what particular events *may* have gone before but what *must* have done so; they would find it as necessary to take all consequents into account on every occasion as all antecedents, and therefore to make their definitions of the cause and the effect precisely correspondent. There would then be neither more nor less occasion for the discussion of Plurality of Causes than for that of Plurality of Effects[1].

---

[1] Conceive, for instance, that the stream of time were, so to say, simply reversed, no change being introduced into the nature of the successive events in themselves. Causes and effects, as we now have them, would simply change places; but there would be no difference in their mutual relations to one another if all the items which compose them were taken into account.

---

[1] To take a familiar instance, a criminal trial (say on a charge of murder) obviously turns on the assumption that, directly we have been able sufficiently to specify the effect, only one particular cause is possible; otherwise no conviction would be justifiable.

§ 9. The account, then, of the causal connexion which explains it as consisting in invariable sequence, seems at bottom merely an improvement of the popular view. It is no doubt a great improvement, and involves conditions which could never have been impressed upon it by any but really scientific minds, but it still needs some correction to fit it for philosophic purposes. It is not, as already remarked, that the statements which it involves are in any way untrue as hypotheses. Hardly any one at the present day would doubt that over the range of all physical phenomena we might confidently assert that *if* any set of antecedents recurred the corresponding set of consequents would follow. Only they do not recur; and even if they did, all that we could thus gather would in strictness be the instantaneous development of the agencies at work. We should learn, not what things have been or what they will be, but what they are *becoming;* that is we should have their tendencies assigned. As soon as we are removed by any appreciable interval from the point in question we cease to be able to make sure that no counteracting influences shall have interfered in the matter.

The present drift of scientific opinion is, I think, rather tending away from this view. Not that men are growing doubtful about its truth, in the above hypothetical form; on the contrary this particular form of regularity or order is being ever more and more firmly held. But it seems to fall far too short of representing the full complexity of nature. It does not strictly apply, as we have seen, to the actual concrete details of events where these are of rare occurrence or are investigated on any large scale; we find it difficult to make use of it in the case of any slow continuous change; we are under strong temptations, when we found our system upon it, of regarding certain original collocations (say of the planets), or certain groups of natural objects (say the species of animals and plants), as lying beyond the bounds of scientific reach, and being in fact 'uncaused'[1].

§ 10. In order therefore fully to meet the scientific wants of the present day, it would seem that, even if we agree to leave the term 'cause' undisturbed in its accepted signification, some wider term, such as 'uniformity', is also needed, which shall embrace much that the other fails to reach. Since then it is this wider conception, rather than the narrower one of causation, which is the strict objective antithesis to 'chance', some space must be devoted to fixing its meaning. It is difficult to give a clear account of a word of such extensive reach, and impossible to do so in the short space which can be spared for it here, but as an aid to understanding by contrast what is meant by chance, a few words must be devoted to the consideration of it.

§ 11. A sufficiently clear account of what is meant by uniformity or regularity, to suit our present purpose, will be found by defining it rather from a consequence which follows from it than from what it is in itself. We may say that whenever any facts or groups of facts are so connected together either in the way of succession, or of co-existence, and at any interval of time or space from one another, that given the one we could conceivably infer the other with certainty, we have a uniformity. Uniformity on the part of nature, and power of inference on our part, are necessarily correlated, and may indeed be said to be co-extensive. For such purposes as we are here concerned with, this indication may suffice, but in any thorough investigation we should of course have to answer the question how and why this power of inference exists. It is quite possible that on analysis almost every uniformity might prove to be resolvable into, and therefore capable of being expressed in, the formula of causation.

---

[1] See Mill's *Logic*, Bk. III. ch. 5, § 7; ch. 22, § 3.

At least this might be so with all uniformities of sequence, as distinguished from those of co-existence. In this case, however unique the phenomena might be in the concrete and on a large scale, they could be broken up into details which are of constant occurrence, and in reference to which there would therefore be no impropriety in speaking of invariable sequence. However this may be, we need only regard uniformity now as that which carries with it power of inference. We may fail of course to make proper use of this power, owing to the complexity of the details and the limited nature of our faculties; but we cannot help regarding it as existent there, and as demanding nothing but an improvement of essentially the same faculties as we now possess, to enable us to grasp every past, present and future fact of which we desire to take cognisance.

§ 12. What then is the bearing of these conceptions of cause and uniformity upon the nature and existence of chance, that is, upon the fundamental assumptions which underlie the science of Probability? There is usually supposed to be such a strong contrast or antagonism between them, that the writers on our subject, as a rule, feel themselves called upon to protest against the existence of any such thing as chance. It has always been recognized, with more or less clearness, that ignorance of the details as combined with knowledge of the averages is inseparably connected with the notion of probability. Hence arises anxiety lest the admission of this ignorance should be supposed to carry along with it, as the ground of our ignorance, the assumption that the individual events in question can happen without causes to produce them. Hume, for instance, in his short Essay on *Probability*, commences with the remark, "Though there be no such thing as chance in the world, our ignorance of the real cause of any event has the same influence on the understanding, &c." Such cautions are perhaps still more prominent on the part of those who have written express treatises on the subject. De Morgan, for instance, goes so far as to declare that the foundations of the theory of Probability have ceased to exist in the mind that has formed the conception "that anything ever did happen or will happen without some particular reason why it should have been precisely what it was and not anything else." Somewhat similar remarks might be quoted from Laplace and others.

§ 13. In the particular form of the controversy above referred to, and which is mostly found in the region of the natural and physical sciences, the contention that chance and causation are irreconcileable occupies rather a defensive position; the main fact insisted on being that, whenever in these subjects we may happen to be ignorant of the details we have no warrant for assuming as a consequence that the details are uncaused. But this supposed irreconcileability is sometimes urged in a much more aggressive spirit in reference to social enquiries. Here the attempt is often made to prove causation in the details, from the known and admitted regularity in the averages. A considerable amount of controversy was excited a few years ago upon this topic, in great part originated by the vigorous and outspoken support of the necessitarian side by Buckle in his *History of Civilization.*

It should be remarked that in these cases the attempt is sometimes made as it were to startle the reader into acquiescence by the singularity of the examples chosen. Instances are selected which, though they possess no greater logical value, are, if one may so express it, emotionally more effective. Every reader of Buckle's History, for instance, will remember the stress which he laid upon the observed fact, that the number of suicides in London remains about the same, year by year;

and he will remember also the sort of panic with which the promulgation of this fact was accompanied in many quarters. So too the way in which Laplace notices that the number of undirected letters annually sent to the Post Office remains about the same, and the comments of Dugald Stewart upon this particular uniformity, seem to imply that they regarded this instance as more remarkable than many analogous ones taken from other quarters.

That there is a certain foundation of truth in the reasonings in support of which the above examples are advanced, cannot be denied, but their authors appear to me very much to overrate the necessary opposition that exists between the theory of Chances and the doctrine of Causation. As regards first that wider conception of order or regularity which we have termed uniformity, anything which might be called objective chance would certainly be at variance with this in one respect. In Probability ultimate regularity is always postulated; in tossing a die, if not merely the individual throws were uncertain in their results, but even the average also, owing to the nature of the die, or the number of the marks upon it, being arbitrarily interfered with, of course no kind of science would attempt to take any account of it.

§ 14. So much must undoubtedly be granted; but must the same admission be made as regards the succession of the individual events? Can causation, in the sense of invariable succession (for we are here shifting on to this narrower ground), be denied, not indeed without suspicions of scientific heterodoxy, but at any rate without throwing uncertainty upon the foundations of Probability? De Morgan, as we have seen, strongly maintains that this cannot be so. I find myself unable to agree with him here, but this disagreement springs not so much from differences of detail, as from those of the point of view in which we regard the science. He always appears to incline in the opinion that the individual judgment in probability is to admit of justification; that when we say, for instance, that the odds in favour of some event are three to two, that we can explain and justify our statement without any necessary reference to a series or class of such events. It is not easy to see how this can be done in any case, but the obstacles would doubtless be greater even than they are, if knowledge of the individual event were not merely unattained, but, owing to the absence of any causal connection, essentially unattainable. On the theory adopted in this work we simply postulate ignorance of the details, but it is not regarded as of any importance on what sort of grounds this ignorance is based. It may be that knowledge is out of the question from the nature of the case, the causative link, so to say, being missing. It may be that such links are known to exist, but that either we cannot ascertain them, or should find it troublesome to do so. It is the fact of this ignorance that makes us appeal to the theory of Probability, the grounds of it are of no importance.

§ 15. On the view here adopted we are concerned only with averages, or with the single event as deduced from an average and conceived to form one of a series. We start with the assumption, grounded on experience, that there is uniformity in this average, and, so long as this is secured to us, we can afford to be perfectly indifferent to the fate, as regards causation, of the individuals which compose the average. The question then assumes the following form:—Is this assumption, of average regularity in the aggregate, inconsistent with the admission of what may be termed causeless irregularity in the details? It does not seem to me that it would be at all easy to prove that this is so. As a matter of fact the two beliefs have constantly co-existed in the same minds. This may not count for much, but it suggests that if there be a contradiction between them it is by no means palpable and obvious. Millions, for instance, have believed in the general uniformity of the seasons taken one with another, who certainly did not believe in, and would very likely have been ready distinctly to deny, the existence of necessary sequences in the various phenomena which compose what we call a season. So with cards and dice; almost every gambler must have recognized that judgment and foresight are of use in the long run, but writers on chance seem to think that gamblers need a good deal of reasoning to convince them that each separate throw is in its nature essentially predictable.

§ 16. In its application to moral and social subjects, what gives this controversy its main interest is its real or supposed bearing upon the vexed question of the freedom of the will; for in this region Causation, and Fatalism or Necessitarianism, are regarded as one and the same thing.

Here, as in the last case, that wide and somewhat vague kind of regularity that we have called Uniformity, must be admitted as a notorious fact. Statistics have put it out of the power of any reasonably informed person to feel a moment's hesitation upon this point. Some idea has already been furnished, in the earlier chapters, of the nature and amount of the evidence which might be furnished of this fact, and any quantity more might be supplied from the works of professed writers upon the subject. If, therefore, Free-will be so interpreted as to imply such essential irregularity as defies prediction both in the average, and also in the single case, then the negation of free-will follows, not as a remote logical consequence, but as an obvious inference from indisputable facts of experience.

Few persons, however, would go so far as to interpret it in this sense. All that troubles them is the fear that somehow this general regularity may be found to carry with it causation, certainly in the sense of regular invariable sequence, and probably also with the further association of compulsion. Rejecting the latter association as utterly unphilosophical, I cannot even see that the former consequence can be admitted as really proved, though it doubtless gains some confirmation from this source.

§ 17. The nature of the argument against free-will, drawn from statistics, at least in the form in which it is very commonly expressed, seems to me exceedingly defective. The antecedents and consequents, in the case of our volitions, must clearly be supposed to be very nearly *immediately* in succession, if anything approaching to causation is to be established: whereas in statistical enquiries the data are often widely separate, if indeed they do not apply merely to single groups of actions or results. For instance, in the case of the mis-directed letters, what it is attempted to prove is that each writer was so much the 'victim of circumstances' (to use a common but misleading expression) that he could not have done otherwise than he did under his circumstances; or rather, to put it better, that what he was going to do could have been foreseen by a logician of adequate sagacity. But really no accumulation of figures to prove that the number of such letters remains the same year by year, can have much bearing upon this doctrine, even though they were accompanied by corresponding figures which should connect the forgetfulness thus indicated with some other characteristics in the writers. So with the number of suicides. If 250 people do, or lately did, annually put an end to themselves in London, the fact, as it thus stands by itself, may be one of importance to the philanthropist and statesman, but it needs bringing into much closer relation with psychological elements if it is to convince us that the actions of men are always instances of inflexible order. In fact, instead of having secured our A and B here in closest intimacy of succession to one another, we find them separated by a considerable interval; often indeed we merely have an A or a B by itself.

§ 18. Again, another deficiency in the reasoning of what, for want of a better name, may be called the School of Buckle, seems to be the laying undue weight upon the mere regularity or persistency of the statistics. These may lead to very important results, but they are not exactly what is wanted for the purpose of proving anything against the freedom of the will; it is not indeed easy to see what connection this has with such facts as that the annual number of thefts or of suicides remains at pretty nearly the same figure. Statistical uniformity seems to me to establish nothing else, at least directly, in the case of human actions, than it does in that of physical characteristics. Take but one instance, that of the mis-directed letters. We were already aware that the height, weight, chest-measurement, and so on, of a number of persons preserved a tolerably regular average amidst innumerable deflections, and we were prepared by analogy to anticipate the same regularity in their mental characteristics. All that we gain, by counting the number of letters which are posted without addresses, is a certain amount of tolerably direct evidence that this is the case. Just as observations of the former kind had already shown that the strength and stature of the human body grouped itself about a mean, so do those of the latter that a similar state of things prevails in respect of the readiness and general trustworthiness of the memory. The evidence is not so direct and conclusive in the latter case, for the memory is not singled out and subjected to measurement by itself, but is taken in combination with innumerable other influencing circumstances[1]. Still there can be little doubt that the statistics tell on the whole in this direction, and that by duly varying and accumulating them they may obtain considerable probative force.

§ 19. If we wish to prove that human conduct is swayed by circumstances, what we want is not so much steady persistency of statistical details, as occasional variations in them. By finding that the number of thefts or murders remains nearly the same year by year, we learn little or nothing about the question at issue; but if we were to find that every alteration in their number was connected with some alteration in such antecedent or concurrent circumstances as the vigilance of the police, the price of food, or the severity of the climate, we should then begin to entertain a strong belief that such circumstances as these were at any rate part-causes in the production of the recorded phenomena. Buckle did certainly call attention to this, as when he maintained that the number of marriages was swayed by the price of corn, but many of the writers who thus appeal to statistics seem to me to entertain rather confused ideas as to what it is which their figures are fitted to establish.

§ 20. It may be as well to remark here in passing, (since to question the validity of an argument is often supposed to imply doubt or disbelief about the truth of the conclusion), that no opinion is here expressed for or against the truth of the doctrine of free-will, in any psychological or metaphysical sense. We are in the region of logic, and all that we are concerned with is capability of inference. It has been attempted to show that although statistical uniformity, and causation, (in the sense of invariable sequence), are two perfectly distinct things; and although it is difficult to show that they are so necessarily connected that the admission of the second must follow from the acceptance of the first; it is nevertheless true that they have some bearing upon one another. There can be little doubt on the whole that the statisticians have added force to the arguments which are commonly considered to disprove free-will, that is, they have helped to establish the conclusion that the actions of men are in every case abstractedly predictable; and that in the region of mind, as in that of external nature, wherever the same antecedents should recur, so would the same consequents follow. Whether, and how far such admissions are hostile to anything that the staunchest ontologist can desire to retain, there is no space to enquire here; though I cannot help remarking in passing that such hostility seems to me to be far too readily urged and accepted by the two parties respectively in the controversy.

---

[1] It seems to me therefore that Mill (*Logic*, Bk. VI. ch. 11, § 1) speaks far too strongly when he says of these examples that they afford "a felicitous verification *à posteriori* of the law of causation in its application to human conduct."

§ 21. So much then for the antithesis between chance and causation. This could be discussed on purely phenomenal grounds. We had nothing else to do than to compare the nature of the succession of the events implied in each case, and to see whether these were necessarily incompatible with one another. In turning now to examine the other antithesis mentioned at the beginning of this chapter, namely that between chance, and choice or design, we have to shift our point of view, and examine not the mere connection of the events in the two cases, but rather the nature of the agencies by which they are brought about. It would be foreign to the purpose of this work to enter at any length into these enquiries, but some notice of them is imperatively necessary in order to clear up a few very prevalent sources of error and confusion.

Of such errors some will seem very obvious and foolish at the present day, at least to any one with the slightest scientific training; but they have played a more or less prominent part in the history of the subject, and are by no means altogether extinct even now. For instance there is an objection to the Science of Probability, which was once popular, and presumably still lingers in some quarters, which charges it with referring events to *chance.* In one sense this is an obvious truism; but of course more than this is meant, though it is by no means easy to say what. If we spelt the word with a capital C, and considered that it was representative of some distinct creative or administrative agency, we should, it may be presumed, be guilty of some form of Manichæism. The only rational meaning, however, of the objection would appear to be that the principles of the science compel us to assume that events (some events, only, that is) happen without causes, and are thereby removed from any direct control of the Deity. As already pointed out, this is altogether a mistake. The Science of Probability makes no assumption whatever about the way in which events are brought about, whether by causation or without it. All that we do is to establish and explain a body of rules which are applicable to classes of cases in which we do not or cannot make inferences about the individuals; but we may avoid committing ourselves to any opinion as to the ground of our inability to make such inferences. The objection therefore has to be somewhat differently stated, and appears finally to reduce itself to this;—that the assumptions upon which the Science of Probability rests, and in consequence of which it is employed, are not inconsistent with a disbelief in causation within certain limits, causation being of course understood simply in the sense of regular sequence. Stated in these modest terms the objection would, I think, be perfectly valid, or rather the facts on which it is based must be admitted; though what connection there could be between such lack of causation and absence of Divine superintenden or presence of some antagonistic agency, it is not easy to s and must indeed be left to any who urge such an objecti to explain.

§ 22. There is another form of objection almost exac the reverse of this, which will also appear to most persons the present day so unreasonable that there is some difficul in understanding precisely what is meant by it. As it h already been alluded to in an extract from Laplace, we ne simply refer back again to the example in Ch. IV. § 14. A m is supposed to have observed the fact that male and fem births occur in the long run in a tolerably steady, and nea equal, ratio to one another, and he has attributed this rangement to Providence. All that need be meant, or co monly is meant, by this is that, on recognizing this fact, formed the conclusion that the Creator had purposely arranged our frames that the births should occur on average in this ratio. He then studies Probability, especi Bernoulli's theorem, and ascertains that he had been lab ing under a mistake, for that these proportional numb were in reality nothing but 'the development of the spective probabilities.' As has been already stated, if by probabilities be meant the proportions, the assertion wo be obviously tautological. If, on the other hand, it be me that there is something in our bodily frames or constituti by which the particular births are so brought to pass that the average they occur about equally often, the perfectly and conclusive reply might be made that this partic arrangement, like everything else in nature, had been duced mediately or immediately by the Creator[1]. The of our not being able to ascertain what this arrangement is, does not affect the argument either way. The physiologist of course believed already that there was something in our bodily constitutions by which each particular birth was determined. Unless the statistical results lead him by some means to discover what this law is, he has discovered really nothing more by means of the figures than what he might equally have known beforehand. He has got a statistical result in the concrete, but it has not so far aided him in analysing it into its causes or antecedents.

---

[1] I am only speaking here of what seems to me logically demanded. As a fact it cannot be denied that every extensive attempt at explanation by 'secondary causes' does to m if not to most minds, seem an vasion of the peculiar province o Deity.

§ 23. Laplace doubtless alluded to a discussion originated by a memoir by Dr Arbuthnott (*Phil. Tr.*, Vol. XXVII.), entitled "An Argument for Divine Providence, taken from the constant Regularity observed in the Births of both sexes." Arbuthnott argued that the observed equality in the number of male and female births was "not the effect of chance, but Divine Providence, working for a good end." All that Arbuthnott showed, however, was that the preponderance of male over female births was too great for it to be reasonably considered an *even* chance that any particular birth was male. We have already maintained (Ch. IV. § 14) that, in our present ignorance of the causes, this statement cannot be regarded as anything more than a truth of definition; assuming that the statistics are true and sufficiently extensive. The chance *is* what statistics show us, and nothing else. Again, the antithesis here drawn between Chance and Providence is unfortunate. Human precautions cannot indeed make a good chance series, but no one will put such a limitation on the Deity, nor need they (one would think) regard the existence of such a series as raising the slightest presumption against providential arrangement. As a matter of fact, the succession of male and female births or deaths is one that does indistinguishably resemble what may be termed a genuine

[illegible]

[illegible] utterly unreasonable to propose [illegible] because the sequence of [illegible] their general tendency towards happiness or misery, good or evil,—is brought about in a way different from what he had expected; whether it be by displaying order where he had expected irregularity, or by revealing the machinery of secondary causes where he had expected immediate agency.

§ 24. It is both amusing and instructive to consider what very different feelings might have been excited in our minds by this co-existence of, what may be called, ignorance of individuals and knowledge of aggregates, if they had presented themselves to our observation in a reverse order. Being utterly unable to make assured predictions about a single life, or the conduct of individuals, people are sometimes startled, and occasionally even dismayed, at the unexpected discovery that such predictions can be confidently made when we are speaking of large numbers. And so some are prompted to exclaim, This is denying Providence! it is utter Fatalism! But let us assume, for a moment, that our familiarity with the subject had been experienced, in the first instance, in reference to the aggregates instead of the individual lives. It is difficult, perhaps, to carry out such a supposition completely; though we may readily conceive something approaching to it in the case of an ignorant clerk in a Life Assurance Office, who had never thought of life, except as having such a 'value' at such an age, and who had hardly estimated it except in the form of averages. Might we not suppose him, in some moment of reflectiveness, being astonished and dismayed at the sudden realization of the utter uncertainty in which the single life is involved? And might not his exclamation in turn be, Why this is denying Providence! It is utter Chaos and Chance! A belief in a Creator and Administrator of the world is not confined to any particular assumption about the nature of the immediate sequence of events, but those who have been accustomed hitherto to regard the events under one of the aspects above referred to, will often for a time feel at a loss how to connect them with the other.

§ 25. The controversies touched upon above refer rather to the province of theology. Confining ourselves to strictly scientific considerations, much of what might be said in this place has been already anticipated in the second and third chapters, so that nothing remains but to add the few remarks which the context seems to demand. In discussing the physical processes by which our series are naturally produced, it was pointed out that, where these series were brought about by human agency, such agency was always found to be removed several stages back from the phenomena finally presented for calculation. Direct human choice or design is, in fact, utterly hostile to the formation of such successions as are wanted for the purposes of Probability; so hostile indeed that it would be found exceedingly difficult to procure them in this way, unless indeed we carefully copied those that had been brought about in the usual fashion.

§ 26. It may however very reasonably be objected that when, in popular phraseology, we ask antithetically whether such and such an event was produced by chance or by design, we are not thinking of any series of events, but merely of the intentions of the agent in that individual instance. This is quite true, but if such intention were constantly acted on, it would show itself in the nature of the resultant series, and in no other way with which we are concerned. A die turns up six. If this is thrown 'by chance' it will so turn up once in about six times. If it was done by design it will always turn up six, supposing sixes were what was wanted. Of course the thrower might equally secure sixes once only in six times, if that had been his wish. But what he would find it exceedingly difficult to do is to secure getting his sixes once in six times, with a sort of series at all resembling that of the random thrower. In fact it would not be possible for him to continue to do it without a considerable amount of calculation, or frequent rectification of his results.

It is therefore easy enough, as a rule, provided we can get a sufficient number of instances, to detect whether any particular result is a product of chance or of design. The former betrays its existence by its merely ultimate tendency towards regularity (assuming, of course, the predominance of what were called in Chap. III. 'the fixed causes'), the latter either by permanent accuracy or by capricious irregularity.

§ 27. When however a single instance only is presented to us, and we are asked to form a judgment upon it, we shall most frequently find ourselves exposed to the hopeless difficulties and ambiguities pointed out in the sections which treated of Inverse Probabilities. Suppose, for instance, we enter a room where a party of gamblers have been pursuing their amusements. We see on the table four dice all lying with the ace uppermost: was this the final throw of a game, or did some one purposely place them so? To decide this question, what we have to do is to compare the relative frequency with which the two kinds of cause would produce such a result. The probability of getting it 'by chance' is easy enough to assign. The number of combinations possible with four dice is 6^4 or 1296; out of these we may fairly say that six are favourable, for the problem cannot reasonably be presumed to be confined to the contemplation of aces merely; any other face, if found uppermost on them all, would equally have given rise to the same question[1]. Hence the chance of finding one and the same face uppermost is $\frac{6}{1296}$ or $\frac{1}{216}$. What, on the other hand, is the chance of getting such a result by design? No one can venture to say, for there are two factors which we cannot estimate. In the first place were the players likely to handle them in any way but with the proper box? The likelihood is decidedly less if they were steady old gamblers than if they were children. In the second place, supposing that they did finger them purposely, were they likely to set them all the same way uppermost? Again, we cannot give an answer, beyond saying vaguely, that it does not seem unlikely that they should do so. The problem is therefore clearly insoluble; but yet we must admit that something like the germ of a solution exists there. As regards the chances in favour of a designed arrangement, we cannot get so near an estimate even as to say whether they are to be reckoned at $\frac{1}{100}$ or at $\frac{1}{1000}$; but we should scruple perhaps to put them as low as one in a million. That is, if *eight* dice were so found, we should begin to feel some commencing confidence that the result was not brought about by an ordinary toss. But any attempt at more definite valuation would be idle[1].

---

[1] It must be remarked that we can merely undertake to say what throws are *commonly regarded* as orderly or artificial, or in other words coincidences; it is scarcely possible to say of any given throw, *in itself*, that it is so. Every succession of faces is arranged according to some law, and we have no right to claim for any one such law that it is more simple than another. The repetition of the same face four times is so recognized, but so might a succession of the first four natural numbers, or two repetitions of any two, or so on. In many games of cards, for instance, a particular pair of cards has a conventional significance assigned to its occurrence, which at once puts it on the same footing as any of the more commonly recognized conjunctions. When therefore we are contrasting

---

chance and design, the 'order' which implies the latter is necessarily assigned mainly by convention. This is very well brought out by Cournot (*Essai sur les fondements de nos connaissances.* Vol. I. p. 71).

[1] Considerations of this kind were introduced into the celebrated De Ros trial, though naturally without any attempt at definite numerical estimation. In so far as the evidence was indirect there seem to have been two main grounds of suspicion; one that Lord De Ros, when dealing at whist, obtained far more court cards than chance would usually assign, the other that for several years in succession his average gains were decidedly large. Against the latter ground of suspicion instances were brought forward by the counsel for the defence, proving that still larger gains had been secured by other players, with no suspicion of unfairness; presumably in order to establish that the gains of Lord De Ros were within the limits of what might be attributed to chance. Of

§ 28. But if the problem of deciding between design and chance becomes so soon insoluble in cases where human design only is supposed to be taken into account, we may fairly say that in cases where this condition is absent it often does not ever approximate to being soluble at all. We have already (Ch. VI. § 17) noticed the dispute whether the distribution of the stars is to be regarded as 'random' or 'designed'; we may now conclude with the few remarks necessary to complete all that need be said upon the subject.

After the removal of various obscurities and ambiguities, it seems to me that the following two questions, amongst others, emerge as being at once capable of being understood, and, to some extent, tested:—(I.) The stars being distributed through space, some of them would of course be nearly in a straight line behind others when looked at from our planet. Supposing that they were tolerably uniformly distributed, we could calculate about how many of them would thus be seen in apparent close proximity to one another. The question is then put, Are there more of them near to one another than such calculation would account for? The answer is that there are many more. So far as I can see the only direct inference that can be drawn from this is that they are *not* uniformly distributed, but have a tendency to go in pairs. This, however, is a perfectly sound and reasonable application of the theory. Any further conclusions such as that these pairs of stars will form systems, as it were, to themselves, revolving about one another, and for all practical purposes unaffected by the rest of the sidereal system, are of course derived from astronomical considerations. Probability confines itself to the simple answer that the distribution is not uniform; it cannot pretend to say whether, and by what

---

course a margin had to be allowed also for superior skill, the game not being one of pure chance. (See the *Times*' report, Feb. 11, 1837).

physical process, these binary systems of stars have been 'caused'[1].

§ 29. (II.) The second question is this, Does the distribution of the stars, after allowing for the case of the binary stars just mentioned, resemble that which would be produced by human agency sprinkling things 'at random'? (We are speaking, of course, of their distribution as it appears to us, on the visible heavens, for this is nearly all that we can observe; but if they extend beyond the telescopic range in every direction, this would lead to practically much the same discussion as if we considered their actual arrangement in space.) We have briefly discussed, in a former chapter, the meaning of 'randomness.' Applying it to the case before us, the question becomes this, Is the distribution tolerably uniform on the whole, but with innumerable individual deflections? That is, when we compare large areas, are the ratios of the number of stars in each equal area approximately equal, whilst, as we compare smaller and smaller areas, do the relative numbers become more and more irregular? With certain exceptions, such as that of the Milky Way and other nebular clusters, this seems to be pretty much the case, at any rate as regards the bulk of the stars[2]. Doubts

---

[1] In this discussion, writers often speak of the probability of a "*physical connection*" between these double stars. The phrase seems misleading, for on the usual hypothesis of universal gravitation *all* stars are physically connected, by gravitation. It is therefore better, as above, to make it simply a question of relative proximity, and to leave it to astronomy to infer what follows from unusual proximity.

[2] Professor Forbes in the paper in the Philosophical Magazine already referred to (Ch. VI. § 17) has given several diagrams to show what, in some given cases, were the actual arrangements of a random distribution. He scattered peas over a chess-board, and then counted the number which rested on each square. His figures seem to show that the general appearance of the stars is much the same as that produced by such a plan of scattering.

Some recent investigations by Mr R. A. Proctor seem to show, however, that there are at least two have indeed been expressed as regards certain constellations (it was in reference to the Pleiades that Michell estimated the odds as 500,000 to 1 against random arrangement). There is however, to my mind, the serious objection against assigning the slightest value to any such conclusion, that the numbers of the stars in each constellation are far too few to secure the requisite conditions for a judgment in Probability. We may profess to come to some conclusion (in the way just indicated) about the general arrangement of the five or six thousand stars visible to the naked eye, but, when we confine ourselves to the small number which compose a constellation, it seems opposed to the fundamental principles of our science to attempt to form an opinion about their arrangement, unless they were much more closely or peculiarly aggregated than they really seem to be. As a matter of fact modern astronomy seems to discountenance the notion of the various constellations having any sort of mutual connection, beyond their purely relative proximity as seen from the earth. Some of the members of each constellation are approaching us rapidly, others as rapidly receding. There seems no reason either to suppose that they are as a rule nearer to one another than they are to various other stars, or indeed to our own solar system.

exceptions to this tolerably uniform distribution. (1) He has ascertained that the stars are decidedly more thickly aggregated in the Milky Way than elsewhere. So far as this is to be relied on the argument is the same as in the case of the double stars; it tends to prove that the proximity of the stars in the Milky Way is not merely apparent, but actual. (2) He has ascertained that there are two large areas, in the North and South hemispheres, in which the stars are much more thickly aggregated than elsewhere. Here, it seems to me, Probability proves nothing: we are simply denying that the distribution is uniform. What may follow in the way of inferences as to the physical process of causation by which the stars have been disposed is a question for the Astronomer. See Mr Proctor's *Essays on Astronomy*, p. 297. Also a series of Essays in *The Universe and the coming Transits*.

CHAPTER X.

DISCUSSION ON SOME OF THE PRINCIPAL VIEWS AS TO THE NATURE AND PROVINCE OF LOGIC, MATERIAL AND CONCEPTUALIST.

§ 1. WE have already had, on a variety of occasions, to call attention to the broad distinction between what may be termed the objective and the subjective views of Logic, but the extreme importance of some of the questions to which this distinction gives rise in Probability will now require a more detailed discussion of it. It need not however be remarked that,—this not being a treatise on logic in general,—in such a brief survey as we can afford to give here, only a few of the more prominent characteristics can be noticed. In following out such a discussion the best plan will be to take as exponents of the respective views, one or two of the ablest and most consistent authors on each side: in this way we shall be adopting the course most intelligible to the student, and also be more likely to avoid any unfairness in deciding what are to be regarded as the natural consequences of the views in question.

§ 2. It would not, I think, be going too far to say that the principal difficulty in the way of a student of Logic at the present day (at any rate in England) consists not so much in the fact that the chief writers upon the subject contradict one another upon many points—for an opportunity of contradiction implies agreement up to a certain stage—as in the fact that over a large region they really hardly get fairly within reach of one another at all. To quarrel upon specific points people must have at any rate some principles in common, where this is not the case they have little else to do than to make up for the vagueness of their dissent by the vigour with which they give expression to it.

That we have not overrated the magnitude of the divergence between the various systems will be evident from a very few extracts and quotations. Hamilton, by implication rather, and Mansel, formally and explicitly, deny that the subject-matter with which Mill is occupied deserves the name of Logic at all; they regard it as being nothing more than a somewhat arbitrary selection from Physical Science. Mill in turn gives equally conclusive indications from his side. He declares, when discussing the import of propositions, that the Conceptualist view is "one of the most fatal errors ever introduced into the philosophy of logic." Elsewhere he gives criticisms which amount to the retort that those who adopt that view are making Logic nothing more than a somewhat arbitrary selection from Psychology.

§ 3. Before proceeding to work out this distinction into some of its details, let us go back, so to say, to the watershed whence the different views as to the nature and province of Logic must take their rise. Every one, it is to be presumed, will admit that a proposition is a statement in words of a judgment about things[1]. Without the words it is pretty generally agreed that there could be nothing more than the merest germ of thought, if even that; without the judgment expressed by it, it would clearly not be the appropriate action of a rational being; whilst without the reference to things it would of course fail in its main object of communicating knowledge, nor could there be any question raised about its material truth or falsehood.

---

[1] The reader is reminded that we are confining our attention, not entirely to English logicians, but to those who may be considered as influential here. No Hegelian, I presume, would consider what we have taken as our starting-point to be in any way deserving of such a name.

Now each of these three sides of the proposition might conceivably be selected as that which is distinctly characteristic of it, to the exclusion of the others; and since the proposition by analysis leads to terms, and by synthesis to arguments, what holds of the proposition holds equally throughout the entire field of Logic. Hence we should apparently be led to three alternative views as to the general nature of Logic. One of these views however, namely that which lays the stress on the words in which the judgment is couched, need hardly be discussed. It has indeed been maintained by Whately that Logic is concerned with language, and with language only. But he does not adhere to this limitation, as indeed no clear thinker could, for the secondary and dependent nature of language as being a medium of thought, or having reference to facts, is far too prominent to be disregarded. Hence it follows that supporters of this view are under such powerful attraction to one or other of the remaining two, that for all practical purposes we need not take any but these into account.

§ 4. Beginning then with the Conceptualist view, that is, starting with the judgment as above indicated, we must of course take, as the element of the judgment, the concept, for this only belongs to the same, namely the mental order or stratum of things. The concept and the judgment are, so to say, on the same plane; they are homogeneous and comparable the one with the other, whereas to mingle names or propositions with them would be to mix up disparate things.

It may be admitted at once that this view has simplicity as a merit; but let us just see to what lengths and sacrifices the determination to adhere to it will lead us. Taking the concept, which may be best defined as the mental[1] counterpart of a general name, we say that this is the real element of the judgment, that the judgment consists of two concepts standing in some sort of relation to each other. So long as we are concerned with general names this will carry us on tolerably well; but how are we to treat *singular* names? Most people would say that these refer directly to individual objects, that this and nothing else than this is their meaning. But the conceptualist sometimes hesitates to say this, for to do so would be to make a dangerous approach towards subordinating the form to the matter. Accordingly the consistent thinker (and in a question of consistency it is of course to Mansel rather than to Hamilton that we turn) abolishes individual terms altogether. He goes the length of asserting that every proper name is a concept, which is perfectly general in its intrinsic character like all other concepts, and that if it does happen to fit only one individual in the course of time, this is a mere historical fact, and therefore alien to the logician's consideration. By so saying he may be presumed to mean that our mental representation of any given individual, say Socrates, can contain only a limited selection of attributes; that this limited group might possibly be found to recur again elsewhere; that, if it did recur, we should not then be able to discriminate between the two without a fresh resort to the individuals themselves, with a view to obtain fresh attributes for the purpose of distinction; and that to do this would be to go outside the concept, in other words to transgress into the matter instead of keeping to the pure form.

§ 5. Again, it must have struck many readers that the

---

[1] We are admitting for purposes of discussion, the tenability of the Conceptualist doctrine, that is, we are not rejecting the psychological theories or assumptions upon which it rests.

Conceptualist logicians make little or no reference to *belief;* the reason of this is not far to seek. For one thing, belief cannot but have some degree of reference to external objects, but with these no communication whatever is to be held, except of course as the original materials or data of thought. For another thing, where, as here, we are only occupied with necessary inferences, nothing but full belief, absolute or conditional, can intrude itself, and therefore we really need not attend to it at all. It need not come before us here, any more than before the pure mathematician; for like him we are only concerned with what follows from, or is consistent with, something else. Provided the links are necessarily connected together, we do not care how the chain may be fixed at either end. It is only when we deal with Induction and Probability, and the delicate questions raised as to whether there is or is not sufficient ground for belief, that this consideration of belief is raised at all. Accordingly any distinction between real and imaginary notions is rejected, the only distinction recognized being that between the possible and the impossible, the former including every notion (whether or not there be things corresponding to it) which does not involve actual self-contradiction, and the latter those which do involve self-contradiction.

§ 6. Again, Classification in any shape deserving that name, disappears, for it cannot be carried on without some observation and comparison of the objects to be classified. What takes its place is Division, for this is really classification confined to purely formal conditions. But even against this objections may be raised. The only way in which we can divide a class is by separating it into those members which do, and those which do not, possess some attribute. But we clearly cannot tell whether things do or do not possess any assignable attributes except by examination of them, and in purely formal logic this is precluded. It is quite true that division by dichotomy is formally valid, for, whatever be the nature of A, a thing must either be A or not A. But then, as Mansel objects, what makes us think of A rather than of any other attribute in relation to the thing in question? Nothing but a consideration of the matter. Hence, though dichotomy in general, as a principle of division, is sound enough, it has nevertheless to be abandoned, because every particular application of it is suggested by reference to the objects and consequent knowledge of their properties, and of this the logician is jealous to the last extreme.

§ 7. The treatment of Induction moreover is simplified. That any process, so narrow and unproductive as the so-called Perfect Induction, should have acquired that name, and have been accepted on its own merits, is hard to believe. But when the general theory from which it follows is adopted, the question assumes a very different aspect. Let us resolve to stick to the analysis and composition of concepts, and this perfect induction, poor as it is, is the best we can attempt. It does not demand any resort to external nature,—any fresh resort that is,—the concepts originally set before us being sufficient for our purpose. It is as near an approach therefore to ordinary Scientific Induction as can be attained by formulæ which are to hold true whatever be the nature of the particular subject-matter to which they are applied.

§ 8. The foregoing remarks will serve to indicate the nature and extent of the divergence between the two opposed views; but something must now be said upon the two designations, Formal and Conceptualist, which are frequently used as practical synonymes to express them. These terms are obviously distinct in their original signification. 'Formal' has reference to the *limits* of the subject-matter rather than its actual nature. It reminds us that we are confining ourselves to those mental processes, or parts of processes, which a independent of the particular subject-matter, that is, in oth words, which follow from the mere form of expression. 'Co ceptualist,' on the other hand, refers rather to the nature our subject than to its limits; it reminds us that we a occupying ourselves with the consideration of concepts general notions as distinguished from external phenomen The two terms are not indeed in strictness synonymou nor need the principal doctrines implied by them be nece sarily held together. Whately, for instance, is a thoroug formalist, but he shows no predilection for the conceptuali doctrines.

It is true, on the other hand, that the consistent concep tualist is under powerful inducements to adopt the form view, partly on grounds of rigid sequence, but still more o grounds of psychological sympathy. Those who have for an reasons determined to confine themselves to the manipulatio of concepts, will naturally recognize a deep and importan distinction between those mental processes which do not, an those which do, require us to go outside the concept for fres matter in order to carry them on; that is, in other word between those processes which are and which are not forma Add to this the fact that those who occupy the conceptuali standpoint are, as a rule, those who believe in necessary law of thought as an ultimate fact (a connection arising out psychological grounds, into which we have not space to ent here), and we see an additional reason why they should mak a sharp distinction between the two classes of processes whic are and which are not respectively formal. The distinctio between formal and material if admitted cannot but be some importance in any case, though it be little more than distinction of method; but in the case in question it ge taken up by, and resolved into, the far more important di tinction between what is *à priori* and what is merely empirical, and there are therefore additional and far stronger reasons for adhering to it.

§ 9. If we now turn to the opposite or material view of Logic, we find a similar series of mutually connected characteristics. Passing over some of those points which have been sufficiently illustrated already by contrast, let us come to that which admirers of Mill will generally regard as his strongest claim to originality, viz. his peculiar doctrine of the syllogism. I think that we are detracting little, if at all, from his merits by saying that this doctrine seems the natural, simple, and almost necessary outcome of the general view of Logic which he has adopted. It is in fact upon his consistent following out of this view, rather than upon this or that conclusion in particular, that we should rest his real claims to high distinction.

His explanation of the syllogism will be arrived at most simply by referring first to what he says about the nature of the so-called immediate inferences. It may have struck some readers as noteworthy that he refuses to allow them the name of inferences at all. But there is surely a meaning in this, and the disputants on each side are quite consistent in adhering to their own views. Take for instance the proposition 'All men are fallible'; from this we obtain by contraposition and limitation, 'some infallibles are not men'. Now regard these propositions as *judgments*; that is, stop short at the mental process of framing the judgment, instead of going on to the facts about which the judgment is made, and it can hardly be denied that one of them is an inference from the other. They certainly cannot be called one and the same judgment, considering that they have different subjects, different predicates, different quantity, and different quality. And if they are not the same judgment, the latter must surely be an inference from the former. But penetrate to the *facts* to which these judgments refer, and we see at once that they are identical, or, to speak more accurately, the one is a portion of the other. The things are the same, being merely differently grouped by the mind, or looked at from a different point of view. The same remarks will apply to another class of immediate judgments which have given some trouble to logicians; for instance, '*A* is greater than *B*, therefore *B* is less than *A*'. Here also the judgments are distinct, whilst the facts judged are identically the same.

§ 10. Now let us introduce the above distinction into the controversy whether the syllogism is or is not a *petitio principii*, and the dispute seems allayed at once, or at any rate its origin and existence are accounted for. The conclusion regarded as a judgment is unquestionably distinct from the premises so regarded, and therefore from that point of view the ordinary theory seems perfectly tenable. But once let a thinker start with the determination that his propositions shall be regarded as, so to say, bottoming upon facts instead of stopping short at concepts, and there is an obvious incompleteness and difficulty about the old explanation. The conclusion, regarded as an objective fact, *is* the premises, or rather a portion of them. We are accordingly driven to carry our investigations a step further back, and we then perceive that the only stage in the reasoning at which new facts were appealed to instead of merely new judgments about them being made, was in the formation of the major premise. When from a limited number of observed instances we generalize so as to include the whole class to which they belong, we are talking and judging about new facts instead of merely varying our judgments about the old ones. Hence Mill's view readily follows, viz. that it is the major premise which really contains the whole inference, the remaining part of the syllogism consisting merely in identification and interpretation of what had gone before. As an illustration of the fact that this explanation of the syllogism, original and important as it is, is nevertheless that to which a consistent supporter of what we may term the Baconian view of logic would necessarily be led, it may be pointed out that it has received the support of Whewell. He is in radical opposition to Mill on fundamental philosophical principles, but, agreeing with him on the whole as to the nature and province of scientific logic, he agrees in consequence on the point in question.

§ 11. The foregoing remarks will be sufficient to indicate the nature and extent of the divergence between the two views before us. It would of course be far beyond the scope of the present discussion to attempt to decide between their claims, but something may fairly be said about some of their subordinate merits and deficiencies. For the Conceptualist theory the main recommendation is the extreme simplicity and homogeneity of the resultant system. Whatever is done is completely done. Nothing is admitted as demonstration but what is (hypothetically) certain. We have none of those results, so dissatisfying to the lover of speculative accuracy, in which no final decision can be obtained by our mere formulæ, but the settlement of the question has to be abandoned to the judgment and skill of the practised observer. This completeness of result is moreover accompanied by a symmetry of treatment which is very fascinating to many minds. These merits are of course purchased at a heavy cost. In addition to the philosophical difficulties which the system involves, a large number of detailed objections may be raised against it. After the elaborate exposition of these given in Mill's Examination of Hamilton's Philosophy there is no occasion for us to enter upon them here.

§ 12. With regard to the defects of the material view of Logic, those who (like myself) accept it on the whole, will not of course admit that they amount to anything like insurmountable obstacles. Nevertheless their existence must be frankly admitted. They may nearly all be summed up in the charge of vagueness of outline, and uncertainty of result. We cannot lay down a precise line for the limit of logic and logical treatment in general, as distinguished from that of the special sciences. In Definition we are forced to admit that the connotation of terms does not admit of accurate determination, but varies with usage and may be almost entirely altered by scientific discoveries. When challenged to state after how many occurrences the repetition of an event may be confidently expected, or how many instances are required to establish an induction, we are obliged to admit that no definite answer can be given, but that it wholly depends upon the nature of the subject-matter. So with Classification; this is no process which can be performed by rule, but it imperatively requires that sagacity of observation and judgment which only long practice combined with natural aptitude can secure. These difficulties are inherent in all human experience, and therefore no science which attempts to grapple with the facts of experience can avoid them.

§ 13. There is indeed a special difficulty occasionally experienced by the student which must be regarded as irrelevant. Those, for instance, who begin with Mill are not unfrequently puzzled by his statement that Logic has to do with the facts or things themselves rather than with our ideas about them; and they not unnaturally ask, How can he then be an idealist? and if so, is he not grossly inconsistent? The answer, of course, is that since the particular opinion which anyone entertains as to the nature of the external world does not affect his position when dealing with scientific evidence in detail, it need not affect the position of one who deals with such evidence as a whole, viz. the logician. It is a question of Metaphysics which lies behind all evidence, and leaves it for the most part entirely unaffected, at least by any direct contact. The astronomer who calculates that the Sun is 92,000,000 miles distant from the Earth, is not called to account and questioned as to how he reconciles this statement with his metaphysics if he be an idealist; and the logician may fairly claim a like toleration. If he lays it down that names are names of things, not of our ideas of things, that what is the import of a proposition is not the judgment, but the facts to which the judgment refers, we have really no more occasion to pry into his metaphysical opinions than into those which he may happen to hold in Theology.

§ 14. The foundation and ground of Induction is a more serious difficulty; for though, like the last, it cannot by rights claim discussion in logic, it is nevertheless almost impossible so to treat induction as not to provoke some perplexing enquiries. The Conceptualist of course avoids all this, for he is only concerned with that which is strictly necessary, and therefore with such induction only (or what he gives that name to) as is performed formally and necessarily. But the Material logician often finds himself in the position of having raised difficulties by his mode of treating the subject, which almost compel him to commit himself to opinions which cannot be justified from a logical but only from a psychological point of view. Suppose he says (as Mill does) that all our knowledge of the uniformity of nature is derived from Induction. He is at once met by some such objection as this: 'You found that belief upon induction, and yet for every act of induction, even for the very first, you must postulate that belief.' So long as we keep strictly to the province of Logic, no answer, I think, can fairly be given to this objection. Every conscious act of induction must demand and presuppose a belief in the Uniformity of Nature, not indeed necessarily throughout its whole extent, but at any rate over some area, and therefore the belief cannot have grown up from the beginning by a series of such acts. In other words, if the inductive inference and the conviction of the Uniformity of Nature are both to be consciously apprehended, it involves a paralogism to regard them as mutually dependent on one another.

Let the logician, however, be permitted to transgress into psychological enquiries, and an answer seems ready to his hand. It may not be a completely satisfactory one, as indeed nothing final can as yet be looked for concerning the nature of belief, but it will serve to turn the edge of the preceding objection. He may fairly reply that we, or our ancestors, have *acted* upon that Uniformity, as the brute creatures do, and that it was only at a later stage that consciousness awakened, that is, that what we call belief ripened out of mere association and habit. Take the case of one of the more intelligent animals. They undoubtedly act upon the Uniformity of Nature: if they did not, they could not continue to subsist for a day any more than ourselves. Now suppose a gradual dawn of self-consciousness in one of them, and a consequent desire to justify its mental processes. Precisely the same difficulty would then arise when it attempted to give a *reason* for processes which had been so long satisfactorily performed. The fact is, that it is assumed that in Logic, though our processes may be sometimes unconsciously performed, they are nevertheless always capable of being called out into distinct consciousness when we choose. This need not be the case in Psychology, and indeed, on the doctrines of the analytical or association school, can seldom be the case with regard to ultimate principles. Hence the logician, when he attempts to give an account and justification of his proceedings in accordance with his own methods, will occasionally be reduced to the alternative of abandoning difficulties as insoluble, or of giving what will be objected against as involving a paralogism. It would, I think, avoid perplexity if he were frankly to assume or state his psychological premises, and if necessary indicate the kind of justification he would give of them.

§ 15. In the foregoing sections of this chapter we have drawn a sharp distinction between the material and formal views of Logic, founded on the difference between facts and our conceptions of facts. We must now, however, look somewhat more closely into this latter distinction, in order to ascertain how far it can be consistently carried out. In so doing we shall find ourselves led into a very important province of enquiry, which has however attracted remarkably little attention. I can only account for its general neglect from the considerations that the material view of Logic is a comparatively recent one, and that the topics now before us are by no means prominent there, but first assume obvious importance in Probability, when this is similarly treated.

The Conceptualist, as we saw, took for his province the whole of what is conceivable; whether any portion of it was real or merely imaginary being of no concern to him. His field is a uniform and homogeneous one of concepts, the only and sufficient warrant for the admissibility of these concepts being that they do not involve self-contradiction. In *applying* his logic he has of course to guarantee their material truth; but speaking as a pure logician he is only concerned with their conceivability when treated by themselves, and their consistency when combined together.

§ 16. It might appear, therefore, as if the Material logician would have an equally uniform and homogeneous field in the world of facts; that he might claim, so to say, all the solid land whilst the other took all the air. But a moment's consideration will show that his position is by no means so simple as this. Facts regarded objectively are no doubt always of one and the same character, but if we do not know them for certain, the logician's claims to them become of a somewhat shadowy nature; and the question may fairly be raised, Do they deserve the name of facts? There are millions of facts, in their objective sense, that is, conceivable objects of certainty to the human intelligence, which, as things are, no man has ever got to know, or in all likelihood ever will get to know. There are others which past generations have known, or future generations will know, but which are entirely beyond the ken of the present generation. There are others, or what are sometimes for want of another word so termed, which as entertained by us certainly do not deserve the name of facts, but should rather be termed hypotheses, well-founded, plausible, hazardous, or wild, as the case may be.

How are we to apply our distinction between facts and conceptions to all these cases? When we say, with Mill, that what we do in framing a proposition is not to join or disjoin two concepts, but to assert or deny that some subject, viz. some determinate phenomenon, or group of phenomena, possesses some attribute, we shall find it extremely difficult to adhere systematically to this distinction. It is clear and sharp enough in many cases, but it becomes hazy and unsubstantial in others. If any one has doubts upon this point, let him endeavour to settle for himself in what precise respects a 'fact' of which one is uncertain, perhaps indeed very doubtful, differs from a 'conception'.

§ 17. It must not be supposed that the above difficulty is at all a fatal one against the Material view of Logic. That it has to be encountered is indeed the great merit of that view, for the necessity arises out of the determination to grapple with objective facts, and to make our logic not merely a scheme of consistency but an organon for arriving at truth and detecting error. But it would be futile to deny that considerable perplexity is thus created. By aiming at an object so wide and important as the truth of inference in general, we have raised difficulties which need never trouble those who confine themselves to the narrow province of correct deduction.

§ 18. Let us put ourselves in imagination into the position of the Materialist logician at the commencement of his operations, and trace them from the origin. We suppose him then to start with the assumption that there is an indefinitely extensive world of objective phenomena on every side of him. By calling them objective we really need mean nothing more, as already remarked, than that they affect others similarly with himself, so that ultimate agreement about them is a conceivable and attainable goal; it is not requisite that they should be objective in any such metaphysical sense as to negative the Berkeleyan theory. Now out of this world of phenomena a small selection is familiar and certain to him and to many others, and here at any rate the distinction between facts and our conceptions about facts is clear and manifest enough. This forms his starting-point in his inductive and deductive enquiries. It comprises what are commonly described as the immediate data of experience, observation, and such like.

Now the general aim and function of the logician seems to be to add to this domain of what is certain, in every direction and to the greatest possible extent. His means for so doing consist mainly of the various methods and resources of Induction, the foundations of which are of course the uniformities of sequence and coexistence by which the different phenomena in question are found to be connected together. In proportion as he obtains more complete mastery over these he is better able to attain his object of adding to the domain of what is known for certain. New facts, as good as those from which he started, can be gradually aggregated around them, thus enabling him to increase what may be termed his area of solid ground.

This extension takes place, of course, to a very different degree in different directions. Of some things very near to us we can attain no certainty whatever with our present resources; of others very remote from us we can attain an almost perfect certainty. What we or others may do next minute, no foresight can predict, any more than what our next neighbour may be thinking of at this moment. On the other hand, the astronomer makes assertions with perfect confidence in reference to stars billions of miles distant; so does the geologist with very considerable confidence in respect of agencies and results removed from us, it may be, millions of years in time. The general result of this is that in one way or another, by the inferences of the various sciences in detail, and therefore in accordance with the laws of the science of Logic as a whole, a vast world of objective phenomena is gradually attained, endowed with all those characteristics of certainty and definiteness which belonged to that little plot of facts which originally lay about us and from which we were supposed to start.

§ 19. Here again then, over all this fresh area thus obtained, the distinction between facts and mere conceptions of facts still holds good. So far therefore nothing seems to arise which is calculated to shake our confidence in the adequacy of such definitions and explanations as involve this distinction. We may still continue to insist upon our account of the reference of names, of the import of propositions, and of all the other characteristics distinctive of the Material or Phenomenalist Logic.

And yet indeed, even within the field of Inductive Logic, signs may be detected of an occasional breaking down of the sharp distinction in question; we may meet now and then with entities (to use the widest term attainable) in reference to which it would be hard to say that they are either facts or conceptions. For instance, Inductive Logic has often occasion to make use of Hypotheses: to which of the above two classes are these to be referred? They do not seem in strictness to belong to either; nor are they, as will presently be pointed out, by any means a solitary instance of the kind.

§ 20. It is true that within the province of Inductive Logic these hypotheses do not give much trouble on this score. However vague may be the form in which they first present themselves to the philosopher's mind, they have not much business to come before us *as logicians* until they are well on their way, so to say, towards becoming facts, until they are beginning to harden into that firm tangible shape in which they will eventually appear. We generally have some such recommendations given to us as that our hypotheses shall be well-grounded, reasonable, or so on. This seems only another way of telling us that however freely or even recklessly the philosopher may make his guesses in the privacy of his own study, he had better not bring them out into public until they can with fair propriety be termed facts with some qualification, 'probable facts', or some such name. The reason, therefore, why we do not take much account of this intermediate state in the hypothesis, when we are dealing with the inductive processes, is that here at any rate it plays only a temporary part; its appearance in that guise is but very fugitive. If the hypothesis be a sound one, it will soon take its place as an admitted fact; if not, it will soon be rejected altogether. Its state as a hypothesis is not a normal one, and therefore we have not much occasion to scrutinize its characteristics. In so saying, it must of course be understood that we are speaking as inductive logicians; the philosopher in his workshop ought to be familiar enough with the hypothesis in every stage of its existence from its earliest origin; but the logician's duty is different, dealing as he does with proof rather than with the processes of original investigation and discovery.

We might indeed even go further, and say that in many cases the hypothesis does not present itself to the reader, that is to the recipient of knowledge, until it has ceased to deserve that name at all. It may be first suggested to him along with the proof which establishes it, he not having had occasion to think of it before. It thus comes at a single step out of the obscurity of the unknown into the full possession of its rights as a fact, skipping practically the intermediate or hypothetical stage altogether. The original investigator himself may have long pondered over it, and kept it present to his mind, in this its dubious stage, but finally have given it to the world with that amount of evidence which raises it at once in the minds of others to the level of commonly accepted facts.

§ 21. Still this doubtful stage exists in every hypothesis, though for logical purposes, and to most minds, it exists in a very fugitive way only. When attention has been directed to it, it may be also detected elsewhere in Logic. Take the case, for instance, of the reference of names. Mill gives the examples of the sun, and a battle, as distinguished from the ideas of them which we, or children, may entertain. Here the distinction is plain and obvious enough. But if, on the other hand, we take the case of things whose existence is doubtful or disputed, the difficulty above mentioned begins to show itself. The case of merely extinct things, or such as have not yet come into existence, offers indeed no trouble, since of course actually *present* existence is not necessary to constitute a fact. The usual distinction may even be retained also in the case of mythical existences. Centaur and Griffin have as universally recognised a significance amongst the poets, painters, and heralds as lion and leopard have. Hence we may claim, even here, that our conceptions shall be 'truthful,' 'consistent with fact,' and so on, by which we mean that they are to be in accordance with universal convention upon such subjects. Necessary and universal accordance is sometimes claimed to be all that is meant by 'objective,' and since universal accordance is attainable in the case of the notoriously fictitious, our fundamental distinction between fact and conception, and our determination that our terms shall refer to what is objective rather than to what is subjective, may with some degree of strain be still conceived to be tenable even here.

§ 22. But when we come to the case of disputed phenomena the difficulty re-emerges. A supposed planet or new mineral, a doubtful fact in history, a disputed theological doctrine, are but a few examples out of many that might be offered. What some persons strenuously assert, others as strenuously deny, and whatever hope there may be of speedy agreement in the case of physical phenomena, experience shows that there is not much prospect of this in the case of those which are moral and historical, to say nothing of theological. So long as those who are in agreement confine their intercourse to themselves, their 'facts' are accepted as such, but as soon as they come to communicate with others all distinction between fact and conception is lost at once, the 'facts' of one party being mere groundless 'conceptions' to their opponents. There is therefore, I think, in these cases a real difficulty in carrying out distinctly and consistently the account which the Materialist logician offers as to the reference of names. It need hardly be pointed out that what thus applies to names or terms applies equally to propositions in which particular or general statements are made involving names.

§ 23. But when we step into Probability, and treat this from the same material or Phenomenal point of view, we can no longer neglect the question which is thus presented to us. The difficulty cannot here be rejected, as referring to what is merely temporary or occasional. The intermediate condition between conjecture and fact, so far from being temporary or occasional only, is here normal. It is just that which is specially characteristic of Probability. Hence it follows that however decidedly we may reject the Conceptualist theory we cannot altogether reject the use of Conceptualist language. If we can prove that a given man will die next year, or attain sufficiently near to proof to leave us practically certain on the point, we may speak of his death as a fact. But if we merely contemplate his death as probable? This is the sort of inference, or substitute for inference, with which Probability is specially concerned. We may, if we so please, speak of 'probable facts,' but if we examine the meaning of the words we may find them not merely obscure, but self-contradictory. Doubtless there are facts here, in the fullest sense of the term, namely the statistics upon which our opinion is ultimately based, for these are known and admitted by all who have looked into the matter. The same language may also be applied to that extension of these statistics by induction which is involved in the assertion that similar statistics will be found to prevail elsewhere, for these also may rightfully claim universal acceptance. But these statements, as was abundantly shown in the earlier chapters, stand on a very different footing from a statement concerning the individual event; the establishment and discussion of the former belong by rights to Induction, and only the latter to Probability.

§ 24. It is true that for want of appropriate terms to express such things we are often induced, indeed compelled, to apply the same name of 'facts' to such individual contingencies. We should not, for instance, hesitate to speak of the fact of the man dying being probable, possible, unlikely, or whatever it might be. But I cannot help regarding such expressions as a strictly incorrect usage arising out of a deficiency of appropriate technical terms. It is doubtless certain that one or other of the two alternatives must happen, but this alternative certainty is not the subject of our contemplation; what we have before us is the *single* alternative, which is notoriously uncertain. It is this, and this only, which is at present under notice, and whose occurrence has to be estimated. We have surely no right to dignify this with the name of a fact, under any qualifications, when the opposite alternative has claims, not perhaps actually equal to, but at any rate not much inferior to its own? Such language, as already remarked, may be quite right in Inductive logic where we are only concerned with conjectures of such a high degree of likelihood, that their non-occurrence need not be taken into practical account, and which are moreover regarded as merely temporary. But in Probability the conjecture may have any degree of likelihood about it; it may be just as likely as the other alternative, nay it may be much less likely. In these latter cases, for instance, if the chances are very much against the man's death, it is surely an abuse of language to speak of the 'fact' of his dying, even though we qualify it by declaring it to be highly improbable. The subject-matter essential to Probability being the uncertain, we can never with propriety employ upon it language which in its original and correct application is only appropriate to what is actually or approximately certain.

§ 25. It should be remembered also that this state of things, thus characteristic of Probability, is *permanent* there. So long as they remain under the treatment of that science our conjectures, or whatever we like to call them, never develop into facts. I calculate, for instance, the chance that a die will give ace, or that a man will live beyond a certain age. Such an approximation to knowledge as is thus acquired is as much as we can ever afterwards hope to get, unless we resort to other methods of enquiry. We do not, as in Induction, feel ourselves on the brink of some experimental or other proof which at any moment may raise it into certainty. It is nothing but a conjecture of a certain degree of strength, and such it will ever remain, so long as Probability is left to deal with it. If anything more is ever to be made out of it we must appeal to direct experience, or to some kind of inductive proof. As we have so often said, individual facts can never be determined here, merely tendencies and averages. I may, indeed, by a second appeal to Probability improve the character of my conjecture, through being able to refer it to a narrower and better class of statistics; but its essential nature remains throughout what it was.

It appears to me therefore that the account of the Materialist view of logic given at the commencement of this chapter, though substantially sound needs some slight reconsideration and restatement. It answers admirably so far as ordinary Induction is concerned, but needs some revision if it is to be equally applicable to that wider view of the nature and processes of acquiring knowledge wherein the science of logic is considered to involve Probability also as well as Induction.

§ 26. Briefly then it is this. We regard the scientific thinker, whether he be the original investigator who discovers, or the logician who analyses and describes the proofs that may be offered, as surrounded by a world of objective phenomena extending indefinitely both ways in time, and in every direction in space. Most of them are, and always will remain, unknown. If we speak of them as facts we mean that they are potential objects of human knowledge, that under appropriate circumstances men could come to determinate and final agreement about them. The scientific or material logician has to superintend the process of converting as much as possible of these unknown phenomena into what are known, of aggregating them, as we have said above, about the nucleus of certain data which experience and observation had to start with. In so doing his principal resources are the Methods of Induction, of which something has been said in a former chapter; another resource is found in the Theory of Probability, and another in Deduction.

Now, however such language may be objected to as savouring of Conceptualism, I can see no better compendious way of describing these processes than by saying that we are engaged in getting at conceptions of these external phenomena, and as far as possible converting these conceptions into facts. What is the natural history of 'facts' if we trace them back to their origin? They first come into being as mere guesses or conjectures, as contemplated possibilities whose correspondence with reality is either altogether disbelieved or regarded as entirely doubtful. In this stage, of course, their contrast with facts is sharp enough. *How* they arise it does not belong to Logic but to Psychology to say. Logic indeed has little or nothing to do with them whilst they are in this form. Everyone is busy all his life in entertaining such guesses upon various subjects, the superiority of the philosopher over the common man being mainly found in the quality of his guesses, and in the skill and persistence with which he sifts and examines them. In the next stage they mostly go by the name of theories or hypotheses, when they are comprehensive in their scope, or are in any way on a scale of grandeur and importance: when however they are of a trivial kind, or refer to details, we really have no distinctive or appropriate name for them, and must be content therefore to call them 'conceptions.' Through this stage they flit with great rapidity in Inductive Logic; often the logician keeps them back until their evidence is so strong that they come before the world at once in the full dignity of facts. Hence, as already remarked, this stage of their career is not much dwelt upon in Logic. But the whole business of Probability is to discuss and estimate them at this point. Consequently, so far as this science is concerned, the explanation of the Material logician as to the reference of names and propositions has to be modified.

§ 27. The best way therefore of describing our position in Probability is as follows:—We are *entertaining a conception* of some event, past, present, or future. From the nature of the case this conception is all that can be actually entertained by the mind. In its present condition it would be incorrect to call it a fact, though we would willingly, if we could, convert it into such by making certain of it one way or the other. But so long as our conclusions are to be effected by considerations of Probability only, we cannot do this. The utmost we can do is to *estimate* or *evaluate* it. The whole function of Probability is to give rules for so doing. By means of reference to statistics or direct deduction, as the case may be, we are enabled to say how much this conception is to be believed, that is in what proportion out of the total number of cases we shall be right in so doing. Our position, therefore, in these cases seems distinctly that of entertaining a conception, and the process of inference is that of ascertaining to what extent we are justified in adding this conception to the already received body of truth and fact.

So long, then, as we are confined to Probability these conceptions remain such. But if we turn to Induction we see that they are meant to go a step further. Their final stage is not reached until they have ripened into facts, and so taken their place amongst uncontested truths. This is their final destination in Logic, and our task is not accomplished until they have reached it.

§ 28. Such language as this in which we speak of our position in Probability as being that of entertaining a conception, and being occupied in determining what degree of belief is to be assigned to it, may savour of Conceptualism, but is in spirit perfectly unconnected with it. Our ultimate reference is always to facts. We start from them as our data, and reach them again eventually in our results whenever it is possible. In Probability, of course, we cannot do this in the individual result, but even then (as shown in Ch. v.) we always *justify* our conclusions by appeal to facts, viz. to what happens in the long run. This being the spirit in which we proceed, it need not further be insisted on that we reject the Conceptualist doctrine and view of Logic.

The discussion which has been thus given to this part of the subject may seem somewhat tedious, but it was so obviously forced upon us when considering the distinction between the two main views of Logic, that it was impossible to pass it over without fear of misapprehension and confusion. Moreover, as will be seen in the course of the next chapter, several important conclusions could not have been properly explained and justified without first taking pains to make this part of our ground perfectly plain and satisfactory.

CHAPTER XI.

CONSEQUENCES OF THE FOREGOING DISTINCTIONS.

§ 1. We are now in a position to explain and justify some important conclusions which, if not direct consequences of the distinctions laid down in the last chapter, will at any rate be more readily appreciated and accepted after that exposition.

In the first place, it will be seen that in Probability *time* has nothing to do with the question; in other words, it does not matter whether the event, whose probability we are discussing, be past, present, or future. The problem before us, in its simplest form, is this:—Statistics (extended by Induction, and practically often gained by Deduction) inform us that a certain event has happened, does happen, or will happen, in a certain way in a certain proportion of cases. We form a conception of that event, and regard it as possible; but we want to do more; we want to know *how much* we ought to expect it (under the explanations given in a former chapter about quantity of belief). There is therefore a sort of relative futurity about the event, inasmuch as our knowledge of the fact, and therefore our justification or otherwise of the correctness of our surmise, almost necessarily comes after the surmise was formed; but the futurity is only relative. The evidence by which the question is to be settled may not be forthcoming yet, or we may have it by us but only consult it afterwards. It is from the fact of the futurity being, as above described, only relative, that I have preferred to speak of the conception of the event rather than of the anticipation of it. The latter term, which in some respects would have seemed more intelligible and appropriate, is open to the objection, that it does rather, in popular estimation, convey the notion of an absolute as opposed to a relative futurity.

§ 2. For example; a die is thrown. Once in six times it gives ace; if therefore we assume, without examination, that the throw is ace, we shall be right once in six times. In so doing we may, according to the usual plan, go *forwards* in time; that is, form our opinion about the throw beforehand, when no one can tell what it will be. Or we might go *backwards;* that is, form an opinion about dice that had been cast on some occasion in time past, and then correct our opinion by the testimony of some one who had been a witness of the throws. In either case the mental operation is precisely the same; an opinion formed merely on statistical grounds is afterwards corrected by specific evidence. The opinion may have been formed upon a past, present, or future event; the evidence which corrects it afterwards may be our own eyesight, or the testimony of others, or any kind of inference; by the evidence is merely meant such subsequent examination of the case as is assumed to set the matter at rest. It is quite possible, of course, that this specific evidence should never be forthcoming; the conception in that case remains as a conception, and never obtains that degree of conviction which qualifies it to be regarded as a 'fact.' This is clearly the case with all past throws of dice the results of which do not happen to have been recorded.

In discussing games of chance there are obvious advantages in confining ourselves to what is really, as well as relatively, future, for in that case direct information concerning the contemplated result being impossible, all persons are on precisely the same footing of comparative ignorance, and must form their opinion entirely from the known or inferred frequency of occurrence of the event in question. On the other hand, if the event be past, there is almost always evidence of some kind and of some value, however slight, to inform us what the event really was; if this evidence is not actually at hand, we can generally, by waiting a little, obtain something that shall be at least of some use to us in forming our opinion. Practically therefore we generally confine ourselves, in anticipations of this kind, to what is really future, and so in popular estimation futurity becomes indissolubly associated with probability.

§ 3. There is however an error closely connected with the above view of the subject, or at least an inaccuracy of expression which is constantly liable to lead to error, which has found wide acceptance, and has been sanctioned by writers of the greatest authority. For instance, both Butler, in his *Analogy*, and Mill, have drawn attention, under one form of expression or another, to the distinction between improbability before the event and improbability after the event, which they consider to be perfectly different things. That this phraseology indicates a distinction of importance cannot be denied, but it seems to me that the language in which it is often expressed requires to be amended.

Butler's remarks on this subject occur in his *Analogy*, in the chapter on miracles. Admitting that there is a strong presumption against miracles (his equivalent for the ordinary expression, an 'improbability before the event') he strives to obtain assent for them by showing that other events, which also have a strong presumption against them, are received on what is in reality very slight evidence. He says, "There is a very strong presumption against common speculative truths, and against the most ordinary facts, before the proof of them; which yet is overcome by almost any proof. There is a presumption of millions to one against the story of Cæsar, or of any other man. For, suppose a number of common facts so and so circumstanced, of which one had no kind of proof, should happen to come into one's thoughts, every one would without any possible doubt conclude them to be false. And the like may be said of a single common fact."

§ 4. These remarks have been a good deal criticized, and they certainly seem to me misleading and obscure in their reference. If one may judge by the context, and by another passage in which the same argument is afterwards referred to[1], it would certainly appear that Butler drew no distinction between miraculous accounts, and other accounts which, to use any of the various expressions in common use, are unlikely or improbable or have a presumption against them; and concluded that since some of the latter were instantly accepted upon somewhat mediocre testimony, it was altogether irrational to reject the former when similarly or better supported[2]. This subject will come again under our notice, and demand fuller discussion, in the chapter on the Credibility of extraordinary stories. It will suffice here to remark that, however satisfactory such a view of the matter might be to some theologians, no antagonist of miracles would for a moment accept it. He would naturally object that, instead of the miraculous element being (as Butler considers) "a small additional presumption" against the narrative, it involved it in a totally distinct class of incredibility; that it multiplied, rather than merely added to, the difficulties and objections in the way of accepting the account.

---

[1] "Is it not self-evident that internal improbabilities of all kinds weaken external proof? Doubtless, but to what practical purpose can this be alleged here, when it has been proved before, that real internal improbabilities, which rise even to moral certainty, are overcome by the most ordinary testimony." Part II. ch. III.

[2] "Miracles must not be compared to common natural events; or to events which, though uncommon, are similar to what we daily experience; but to the extraordinary phenomena of nature. And then the comparison will be between the presumption against miracles, and the presumption against such uncommon appearances, suppose as comets,"……. Part II. ch. II.

Mill's remarks (*Logic*, Bk. III. ch. XXV. § 4) are of a different character. Discussing the grounds of disbelief he speaks of people making the mistake of "overlooking the distinction between (what may be called) improbability before the fact, and improbability after it, two different properties, the latter of which is always a ground of disbelief, the former not always." He instances the throwing of a die. It is improbable beforehand that it should turn up ace, and yet afterwards, "there is no reason for disbelieving it if any credible witness asserts it." So again, "the chances are greatly against A. B.'s dying, yet if any one tells us that he died yesterday we believe it."

§ 5. That there is some difficulty about such problems as these must be admitted. The fact that so many people find them a source of perplexity, and that such various explanations are offered to solve the perplexity, are a sufficient proof of this[1]. The considerations of the last chapter, however, over-technical and even scholastic as some of the language in which it was expressed may have seemed to the reader, will I hope guide us to a more satisfactory way of regarding the matter.

---

[1] For instance, Mr J. F. Stephen explains it by drawing a distinction between chances and probabilities, which he says that Butler has confused together; "the objection that very ordinary proof will overcome a presumption of millions to one is based upon a confusion between probabilities and chances. The probability of an event is its capability of being proved. Its chance is the numerical proportion between the number of possible cases—supposed to be equally favourable—favourable to its occurrence; and the number of possible cases unfavourable to its

When we speak of an improbable event, it must be remembered that, objectively considered, an event can only be more or less *rare;* the extreme degree of rarity being of course that in which the event does not occur at all. Now, as was shown in the last chapter, our position, when forming judgments of the kind in question, is that of entertaining a conception or conjecture (call it what we will), and assigning a certain weight of trustworthiness to it. The real distinction, therefore, between the two classes of examples respectively, which are adduced both by Butler and by Mill, consists in the way in which those conceptions are obtained; they being obtained in one case by the process of guessing, and in the other by that of giving heed to the reports of witnesses.

§ 6. Take Butler's instance first. In the 'presumption before the proof' we have represented to us a man thinking of the story of Cæsar, that is, making a guess about certain historical events without any definite grounds for it, and then speculating as to what value is to be attached to the probability of its truth. Such a guess is of course, as he says, concluded to be false. But what does he understand by the 'presumption after the proof'? That a story not adopted at random, but actually suggested and supported by witnesses, should be true. The latter might be accepted, whilst the former would undoubtedly be rejected; but all that this proves, or rather illustrates, is that the testimony

occurrence" (*General view of the Criminal Law of England,* p. 255). Donkin, again (*Phil. Magazine,* June, 1851), employs the terms improbability and incredibility to mark the same distinction.

of almost any witness is in most cases vastly better than a mere guess[1]. We may in both cases alike speak of 'the event' if we will; in fact, as was admitted in the last chapter, common language will not readily lend itself to any other way of speaking. But it should be clearly understood that, phrase it how we will, what is really present to the man's mind, and what is to have its probable value assigned to it, is the conception of an event, in the sense in which that expression has already been explained. And surely no two conceptions can have a much more important distinction put between them than that which is involved in supposing one to be an unsupported guess, and the other the report of a witness. Precisely the same remarks apply to the example given by Mill. Before A. B.'s death our opinion upon the subject was nothing but a guess of our own founded upon life statistics; after his death it was founded upon the evidence of some one who presumably had tolerable opportunities of knowing what the facts really were.

§ 7. That the distinction before us has no essential connection whatever with time is indeed obvious on a moment's consideration. Conceive for a moment that some one had opportunities of knowing whether A. B. would die or not. If he told us that A. B. would die to-morrow, we should in that case be just as ready to believe him as when he tells us that A. B. *has* died. If we continued to feel any doubt about the statement (supposing always that we had full confidence about his veracity in matters into which he had duly enquired), it would be because we thought that in his case, as in ours, it was equivalent to a guess, and nothing more. So with the event when past, the fact of its being past makes no difference whatever; until the credible witness informs us of what he knows to have occurred, we should doubt it if it happened to come into our minds, just as much as if it were future.

---

[1] In the extreme case of the witness himself merely guessing, or being as untrustworthy as if he merely guessed, the two stories will of course stand on precisely the same footing. This case will be noticed again in Chapter XVII. It may be remarked that there are several subtleties here which cannot be adequately noticed without some previous investigation into the question of the credibility of witnesses.

The distinction, therefore, between probability before the event and probability after the event seems to resolve itself simply into this;—before the event we often have no better means of information than to appeal to statistics in some form or other, and so to guess amongst the various possible alternatives; after the event the guess may most commonly be improved or superseded by appeal to specific evidence, in the shape of testimony or observation. Hence, naturally, our estimate in the latter case is commonly of much more value. But if these characteristics were anyhow inverted; if, that is, we were to confine ourselves to guessing about the past, and if we could find any additional evidence about the future, the respective values of the different estimates would also be inverted. The difference between these values has no necessary connection with time, but depends entirely upon the different grounds upon which our conception or conjecture about the event in question rests.

§ 8. The following imaginary example will serve to bring out the point indicated above. Conceive a people with very short memories, and who preserved no kind of record to perpetuate their hold upon the events which happened amongst them. The whole region of the past would then be to them what much of the future is to us; viz. a region of guesses and conjectures, one in reference to which they could only judge upon general considerations of probability, rather than by direct and specific evidence. But conceive also that they had amongst them a race of prophets who could succeed in foretelling the future with as near an approach to accuracy and trustworthiness as our various histories, and memories, and recollections, can attain in respect to the past. The present and usual functions of direct evidence or testimony, and of probability, would then be simply inverted; and so in consequence would the present accidental characteristics of improbability before and after the event. It would then be the latter which would by comparison be depreciated, and regarded as 'not always a ground of disbelief,' whereas in the case of the former we should then have it maintained that it always was so.

§ 9. The origin of the mistake just discussed is worth enquiring into. I take it to be as follows. It is often the case, as above remarked, when we are speculating about a future event, and almost always the case when that future event is taken from a game of chance, that all persons are in precisely the same condition of ignorance in respect to it. The limit of available information is confined to statistics, and amounts to the knowledge that the unknown event must assume some one of various alternative forms. The conjecture, therefore, of any one man about it is as valuable as that of any other. But in regard to the past the case is very different. Here we are not in the habit of relying upon statistical information. Hence the conjectures of different men are of extremely different values; in the case of many they amount to what we call positive knowledge. This puts a broad distinction, in popular estimation, between what may be called the objective certainty of the past and the future, a distinction, however, which from the standing-point of a science of inference ought to have no existence.

In consequence of this, when we apply to the past and the future respectively the somewhat ambiguous expression 'the chance of the event', it commonly comes to bear very different significations. Applied to the future it bears its proper meaning, namely, the value to be assigned to a conjecture upon statistical grounds. It does so, because in this case hardly any one has more to judge by than such conjectures. But applied to the past it shifts its meaning, owing to the fact that whereas some men have conjectures only, others have positive knowledge. By the chance of the event is now often meant, not the value to be assigned to a conjecture founded on statistics, but to such a conjecture derived from and enforced by any body else's conjecture, that is by his knowledge and his testimony.

§ 10. There is a class of cases in apparent opposition to some of the statements in this chapter, but which will be found, when examined closely, decidedly to confirm them. I am walking, say, in a remote part of the country, and suddenly meet with a friend. At this I am naturally surprised. Yet if the view be correct that we cannot properly speak about events in themselves being probable or improbable, but only say this of our conjectures about them, how do we explain this? We had formed no conjecture beforehand, for we were not thinking about anything of the kind, but yet few would fail to feel surprise at such an incident.

The reply might fairly be made that we *had* formed such anticipations tacitly. On any such occasion every one unconsciously divides things into those which are known to him and those which are not. During a considerable previous period a countless number of persons had met us, and all fallen into the list of the unknown to us. There was nothing to remind us of having formed the anticipation or distinction at all, until it was suddenly called out into vivid consciousness by the exceptional event. The words which we should instinctively use in our surprise seem to show this:—'Who would have thought of seeing you here?' viz. Who would have given any weight to the latent thought if it had been called out into consciousness beforehand? We put our words into the past tense, showing that we have had the distinction lurking in our minds all the time. We always have a multitude of such ready-made classes of events in our minds, and when a thing happens to fall into one of those classes which are very small we cannot help noticing the fact.

Or suppose I am one of a regiment into which a shot flies, and it strikes me, and me only. At this I am surprised, and why? Our common language will guide us to the reason. 'How strange that it should just have hit *me* of all men!' We are thinking of the very natural two-fold division of mankind into, ourselves, and every body else; our surprise is again, as it were, retrospective, and in reference to this division. No anticipation was distinctly formed, because we did not think beforehand of the event, but the event, when it has happened, is at once assigned to its appropriate class[1].

§ 11. This view is confirmed by the following considerations. Tell the story to a friend, and he will be a little surprised, but less so than we were, *his* division in this particular case being,—his friends (of whom we are but one), and the rest of mankind. It is not a necessary division, but it is the one which will be most likely suggested to him.

Tell it again to a perfect stranger, and his division being different (viz. we falling into the majority) we shall fail to make him perceive that there is anything at all remarkable in the event.

---

[1] I have since seen a similar explanation by De Morgan in the *Camb. Phil. Transactions*, Vol. IX. p. 119.

It is not of course attempted in these remarks to justify our surprise in every case in which it exists. Different persons might be differently affected in the cases supposed, and the examples are therefore given mainly for illustration. Still on principles already discussed (Ch. v. § 32) we might expect to find something like a general justification of the amount of surprise.

§ 12. The answer commonly given in these cases is confined to attempting to show that the surprise should not arise, rather than to explaining how it does arise. It takes the following form,—'You have no right to be surprised, for nothing remarkable has really occurred. If this particular thing had not happened something equally improbable must. If the shot had not hit you or your friend, it must have hit some one else who was *à priori* as unlikely to be hit.'

For one thing this answer does not explain the fact that almost every one *is* surprised in such cases, and surprised somewhat in the different proportions mentioned above. Moreover it has the inherent unsatisfactoriness of admitting that something improbable has really happened, but getting over the difficulty by saying that all the other alternatives were equally improbable. A natural inference from this is that there is a class of things, in themselves really improbable, which can yet be established upon very slight evidence. Butler accepted this inference, and worked it out to the strange conclusion given above. Mill attempts to avoid it by the consideration of the very different values to be assigned to improbability before and after the event. Some further discussion of this point will be found in the chapter on Fallacies, and in that on the Credibility of Extraordinary Stories.

§ 13. In connection with the subject at present under discussion we will now take notice of a distinction which we shall often find insisted on in works on Probability, but to which apparently needless importance has been attached. It is frequently said that probability is *relative,* in the sense that it has a different value to different persons according to their respective information upon the subject in question. For example, two persons, *A* and *B,* are going to draw a ball from a bag containing 4 balls: *A* knows that the balls are black and white, but does not know more; *B* knows that three are black and one white. It would be said that the probability of a white ball to *A* is $\frac{1}{2}$, and to *B* $\frac{1}{4}$.

When however we regard the subject from the material standing point, there really does not seem to me much more in this than the principle, equally true in every other science, that our inferences will vary according to the data we assume. We might on logical grounds with almost equal propriety speak of the area of a field or the height of a mountain being relative, and therefore having one value to one person and another to another. The real meaning of the example cited above is this: *A* supposes that he is choosing white at random out of a series which in the long run would give white and black equally often; *B* supposes that he is choosing white out of a series which in the long run would give three black to one white. By the application, therefore, of a precisely similar rule they draw different conclusions; but so they would under the same circumstances in any other science. If two men are measuring the height of a mountain, and one supposes his base to be 1000 feet, whilst the other takes it to be 1001, they would of course form different opinions about the height. The science of mensuration is not supposed to have anything to do with the truth of the data, but assumes them to have been correctly taken; why should not this be equally the case with

Probability, making of course due allowance for the peculiar character of the data with which it is concerned?

§ 14. This view of the relativeness of Probability is connected, as it appears to me, with the subjective view of the science, and is indeed characteristic of it. It seems a fair illustration of the weak side of that view, that it should lead us to lay any stress on such an expression. As was fully explained in the last chapter, in proportion as we work out the Conceptualist principle we are led away from the fundamental question of the material logic, viz. Is our belief actually correct, or not? and, if the former, to what extent and degree is it correct? We are directed rather to ask, What belief does any one as a matter of fact hold? And, since the belief thus entertained naturally varies according to the circumstances and other sources of information of the person in question, its relativeness comes to be admitted as inevitable, or at least it is not to be wondered at if such should be the case.

On our view of Probability, therefore, its 'relativeness' in any given case is a misleading expression, and it will be found much preferable to speak of the effect produced by variations in the nature and amount of the data which we have before us. Now it must be admitted that there are frequently cases in our science in which such variations are peculiarly likely to be found. For instance, I am expecting a friend who is a passenger in an ocean steamer. There are a hundred passengers on board, and the crew also numbers a hundred. I read in the papers that one person was lost by falling overboard; my anticipation that it was my friend who was lost is but small, of course. On turning to another paper, I see that the man who was lost was a passenger, not one of the crew; my slight anxiety is at once doubled. But another account adds that it was an Englishman, and on that line at that season the English passengers are known to be few; I at once begin to entertain decided fears. And so on, every trifling bit of information instantly affecting my expectations.

§ 15. Now since it is peculiarly characteristic of Probability, as distinguished from Induction, to be thus at the mercy, so to say, of every little fact that may be floating about when we are in the act of forming our opinion, what can be the harm (it may be urged) of expressing this state of things by terming our state of expectation *relative?*

There seem to me to be two objections. In the first place, as just mentioned, we are induced to reject such an expression on grounds of consistency. It is inconsistent with the general spirit and treatment of the subject hitherto adopted, and tends to divorce Probability from Inductive logic instead of regarding them as cognate sciences. We are aiming at truth, as far as that goal can be reached by our road, and therefore we dislike to regard our conclusions as relative in any other sense than that in which truth itself may be said to be relative.

In the second place, this condition of unstable assent, this constant liability to have our judgment affected to any degree at any moment by the accession of new knowledge, though doubtless characteristic of Probability, does not seem to me characteristic of it in its sounder and more legitimate applications. It seems to me rather of the nature of a precipitate judgment formed in accordance with the rules, than a strict example of their natural employment. Such precipitate judgments may occur in the case of ordinary deductive conclusions. In the practical exigencies of life we are constantly in the habit of forming a hasty opinion with nearly full confidence, at any rate temporarily, upon the strength of evidence which we must well know at the time cannot be final. We wait a short time, and something else turns up which induces us to alter our opinion, perhaps to reverse it. Here our conclusions may have been perfectly sound under the given circumstances, that is, they may be such as every one else would have drawn who was bound to make up his mind upon the data before us, and they are unquestionably 'relative' judgments in the sense now under discussion. And yet, I think, every one would shrink from so terming them who wished systematically to carry out the view that Logic was to be regarded as an organon of truth.

§ 16. In the examples of probability which we have hitherto employed, we have for the most part assumed that there was a certain body of statistics set before us on which our conclusion was to rest. It was assumed, on the one hand, that no direct specific evidence could be got, so that the judgment was really to be one of probability, and to rest on these statistics; in other words, that nothing better than them was available for us. But it was equally assumed, on the other hand, that these statistics were open to the observation of every one, so that we need not have to put up with anything inferior to them in forming our opinion. In other words, we have been assuming that here, as in the case of most other sciences, those who have to draw a conclusion start from the same footing of opportunity and information. This, for instance, clearly is or ought to be the case when we are concerned with games of chance; ignorance or misapprehension of the common data is never contemplated there. So with the statistics of life or other insurance: so long as our judgment is to be accurate (after its fashion) or justifiable, the common tables of mortality are all that any one has to go by.

§ 17. It is true that in the case of a man's prospect of death we should each qualify our judgment by what we knew or reasonably supposed as to his health, habits, profession, and so on, and should thus arrive at varying estimates. But no one could *justify* his own estimate without appealing explicitly or implicitly to the statistical grounds on which he had relied, and if these were not previously available to other persons, he must now set them before their notice. In other words, the judgments we entertain, here as elsewhere, are only relative so long as we rest them on grounds peculiar to ourselves. The process of justification, which I consider to be essential to logic, has a tendency to correct such individualities of judgment, and to set all observers on the same basis as regards their data.

It is better therefore to regard the conclusions of Probability as being absolute and objective, in the same sense as, though doubtless in a less degree than, they are in Induction. Fully admitting that our conclusions will in many cases vary exceedingly from time to time by fresh accessions of knowledge, it is preferable to regard such fluctuations of assent as partaking of the nature of precipitate judgments, founded on special statistics, instead of depending only on those which are common to all observers. In calling such judgments precipitate it is not implied that there is any blame in entertaining them, but simply that, for one reason or another, we have been induced to form them without waiting for the possession of the full amount of evidence, statistical or otherwise, which might ultimately be looked for. This explanation will suit the facts equally well, and is more consistent with the general philosophical position maintained in this work.

CHAPTER XII.

ON THE CONCEPTION AND TREATMENT OF MODALITY.

§ 1. THE reader who knows anything of Logic will have perceived before now that we have been touching in a variety of places upon that most thorny and repulsive of districts in the logical territory;—modality. It will be advisable, however, to put together, somewhat more definitely, what has to be said upon the subject. I propose, therefore, to devote this chapter to a brief account of the principal varieties of treatment which the modals have received at the hands of professed logicians.

It must be remarked at the outset that the sense in which modality and modal propositions have been at various times understood, is by no means fixed and invariably the same. This diversity of view has arisen partly from corresponding differences in the view taken of the province and nature of logic, and partly from differences in the philosophical and scientific opinions entertained as to the constitution and order of nature. Another very powerful agent in bringing about a change in the treatment of the subject must be recognized in the gradual and steady growth of the theory of Probability, as worked out by the mathematicians from their own point of view.

§ 2. In spite, however, of these differences of treatment, there has always been some community of subject-matter in the discussions upon this topic. There has almost always been some reference to quantity of belief; enough to justify Prof. De Morgan's[1] remark, that Probability was "the unknown God whom the schoolmen ignorantly worshipped when they so dealt with this species of enunciation, that it was said to be beyond human determination whether they most tortured the modals, or the modals them." But this reference to quantity of belief has sometimes been direct and immediate, sometimes indirect and arising out of the nature of the subject-matter of the proposition. The fact is, that that distinction between the purely subjective and purely objective views of logic, which I have endeavoured to bring out into prominence in the tenth chapter, was not by any means clearly recognized in early times, nor indeed before the time of Kant, and the view to be taken of modality naturally shared in the consequent confusion. This will, I hope, be made clear in the course of the following chapter, which is intended to give a brief sketch of the principal different ways in which the modality of propositions has been treated in logic. As it is not proposed to give anything like a regular history of the subject, there will be no necessity to adhere to any strict sequence of time, or to discuss the opinions of any writers, except those who may be taken as representative of tolerably distinct views. The outcome of such investigation will be, I hope, to convince the reader (if, indeed, he had not come to that conviction before), that the logicians, after having had a long and fair trial, have totally failed to make anything satisfactory out of this subject of the modals by their methods of enquiry and treatment; and that it ought, therefore, to be banished entirely from that science, and relegated to Probability.

§ 3. From the earliest study of the syllogistic process it was seen that, complete as that process is within its own domain, the domain, at any rate under its simplest treatment, is a very limited one. Propositions of the pure form, All (or some) *A* is (or is not) *B*, are found in practice to form but a small portion even of our categorical statements. We are perpetually meeting with others which express the relation of *B* to *A* with various degrees of necessity or probability; e.g. *A* must be *B*, or *A* may be *B*; or the effect of such facts upon our judgment, e.g. I am perfectly certain that *A* is *B*, or I think that *A* may be *B*, with many others of a more or less similar type. The question at once arises, How are such propositions to be treated?

---

[1] *Formal Logic*, p. 232.

§ 4. One very simple plan suggests itself, and has indeed been repeatedly advocated, viz. just to transfer all that is characteristic of such propositions into that convenient receptacle for what is troublesome elsewhere, the predicate[1]. Has not another so-called modality been thus got rid of[2]? and has it not been attempted by the same device to abolish the distinctive characteristic of negative propositions, thus shifting the negative particle into the predicate? It is true that, up to a certain point, something may be done in this way. Given the reasoning, 'Those who take arsenic will probably die; *A* has taken it, therefore he will probably die;' it is easy to convert this into an ordinary syllogism of the pure type, by simply wording the major, 'Those who take arsenic are people-who-will-probably-die,' when the conclusion follows in the same form, '*A* is one who-will-probably-die.' But this device will only carry us a very little way. Suppose that the minor premise also is of the same modal description, e.g. '*A* has probably taken arsenic,' and it will be seen that we cannot relegate the modality here also to the predicate without being brought to a stop by finding that there are four terms in the syllogism.

---

[1] This appears to be the purport of some statements in a very confused passage in Whately's *Logic* (Bk. II., ch. IV. § 1). "A modal proposition may be stated as a pure one by attaching the mode to one of the terms, and the proposition will in all respects fall under the foregoing rules;...'It is probable that all knowledge is useful;' 'probably useful' is here the predicate." He draws apparently no such distinction as that between the true and false modality referred to in the next note. What is really surprising is that even Hamilton puts the two (the true and the false modality) upon the same footing. "In regard to these [the former] the case is precisely the same; the mode is merely a part of the predicate." *Logic*, I. 257.

[2] I allude of course to such examples as '*A* killed *B* unjustly,' in which the killing of *B* by *A* was sometimes said to be asserted not simply but with a modification. (Hamilton's *Logic*, I. 256.) It is obvious that the modification in such cases is by rights merely a part of the predicate, there being no formal distinction between '*A* is the killer of *B*' and '*A* is the unjust killer of *B*.' Indeed some logicians who were too conservative to reject the generic name of modality in this application adopted the common expedient of introducing a specific distinction which did away with its

But even if there were not the above objection, it does not appear that anything is to be gained in the way of intelligibility or method by such a device. For what is meant by a modal predicate, by the predicate 'probably mortal,' for instance, in the proposition 'All poisonings by arsenic are probably mortal'? If the analogy with ordinary pure propositions is to hold good, it must be a predicate referring to the *whole* of the subject, for the subject is distributed. But then we are at once launched into the difficulties discussed in a former chapter (Ch. v. §§ 19—25), when we attempt to

meaning, terming the spurious kind 'material modality' and the genuine kind 'formal modality' (Keckermann, *Systema Logicæ*, Lib. II. ch. 3). It was, I believe, a common scholastic distinction.

Some, again, of the schoolmen drew a distinction between the cases in which the modality belonged to the subject or predicate, and to the copula, employing technical names to signify them (Prantl, *Geschichte der Logik*, III. 130).

For some account of the dispute as to whether the negative particle was to be considered to belong to the copula or to the predicate, see Hamilton's *Logic*, I. 253.

justify or verify the application of the predicate. We have to enquire (at least on the view adopted in this work) whether the application of the predicate 'probably mortal' to the *whole* of the subject, really means at bottom that the predicate 'mortal' is to be applied to a *portion* (more than half) of the members denoted by the subject. When the transference of the modality to the predicate raises such intricate questions as this as to the sense in which the predicate is to be interpreted, there is surely nothing gained by the step.

§ 5. A second, and more summary way of shelving all difficulties of the subject, so far at least as logic, or the writers upon logic, are concerned, is found by simply denying that modality has any connection with logic. This is the course adopted by Hamilton and Mansel, in reference to whom one cannot help remarking that an unduly large portion of their logical writings seems occupied with telling us what does *not* belong to logic. They justify their rejection on the ground that the mode belongs to the matter, and must be determined by a consideration of the matter, and therefore is extralogical. To a certain extent I agree with their grounds of rejection, for (as explained in Chap. v.) it is not easy to see how the degree of modality of any proposition, whether premise or conclusion, can be justified without appeal to the matter. But then questions of justification, in any adequate sense of the term, belong to a range of considerations somewhat alien to Hamilton's and Mansel's way of regarding the science. The complete justification of our inferences is a matter which involves their truth or falsehood, a point with which these writers do not much concern themselves, being only occupied with the consistency of our reasonings, not with their conformity with fact. Were I speaking as a Hamiltonian I should say that modality *is* formal rather than material, for though we cannot justify the degree of our belief of a proposition without appeal to the matter, we can to a moderate degree of accuracy estimate it without any such appeal; and this would seem to be quite enough to warrant its being regarded as formal.

It must be admitted that Hamilton's account of the matter when he is recommending the rejection of the modals, is not by any means clear and consistent. He not only fails, as already remarked, to distinguish between the formal and the material (in other words, the true and the false) modality; but when treating of the former he fails to distinguish between the extremely diverse aspects of modality when viewed from the Aristotelean and the Kantian stand-points. Of the amount and significance of this difference we shall speak presently, but it may be just pointed out here that Hamilton begins (Vol. I. p. 257) by rejecting the modals on the ground that the distinctions between the necessary, the contingent, the possible, and the impossible, must be wholly rested on an appeal to the matter of the propositions, in which he is, I think, quite correct. But then a little further on (p. 260), in explaining 'the meaning of three terms which are used in relation to pure and modal propositions,' he gives the widely different Kantian, or threefold division into the apodeictic, the assertory, and the problematic. He does not take the precaution of pointing out to his hearers the very different general views of logic from which these two accounts of modality spring[1].

§ 6. There is one kind of modal syllogism which it would seem unreasonable to reject on the ground of its not being formal, and which we may notice in passing. The premise 'Any A is probably B,' is equivalent to 'Most A are B.' Now it is obvious that from two such premises as 'Most A are B,' 'Most A are C,' we can deduce the consequence, 'Some C are B.' Since this holds good whatever may be the nature of A, B, and C, it is, according to ordinary usage of the term, a formal syllogism. Mansel, however, refuses to admit that any such syllogisms belong to formal logic. His reasons are given in a rather elaborate review[1] and criticism of some of the logical works of De Morgan, to whom the introduction of 'numerically definite syllogisms' is mainly due. Mansel does not take the particular example given above, as he is discussing a somewhat more comprehensive algebraic form. He examines it in a special numerical example:—18 out of 21 Ys are X[2]; 15 out of 21 Ys are Z; the conclusion that 12 Zs are X is rejected from formal logic on the ground that the arithmetical judgment involved is synthetical, not analytical, and rests upon an intuition of quantity. We cannot enter upon any examination of these reasons here; but it may merely be remarked that his criticism demands the acceptance of the Kantian doctrines as to the nature of arithmetical judgments, and that it would be better to base the rejection not on the ground that the syllogism is not *formal*, but on the ground that it is not *analytical*.

---

[1] He has also given a short discussion of the subject elsewhere (*Discussions*, Ed. II. p. 702), in which a somewhat different view is taken. The modes are indeed here admitted into logic, but only in so far as they fall by subdivision under the relation of genus and species, which is of course tantamount to their entire rejection; for they then differ in no essential way from any other examples of that relation.

§ 7. There is another and practical way of getting rid of the perplexities of modal reasoning which must be noticed here. It is the resource of ordinary reasoners rather than the decision of professed logicians[1], and, like the first method of evasion already pointed out in this chapter, is of very partial application. It consists in treating the premises, during the process of reasoning, as if they were pure, and then reintroducing the modality into the conclusion, as a sort of qualification of its full certainty. When each of the premises is nearly certain, or when from any cause we are not concerned with the extent of their departure from full certainty, this rough expedient will answer well enough. It is, I apprehend, the process which passes through the minds of most persons in such cases, in so far as they reason consciously. They would, presumably, in such an example as that previously given (§ 4), proceed as if the premises that 'those who take arsenic will die,' and that 'the man in question has taken it,' were quite true, instead of being only probably true, and they would consequently draw the conclusion that 'he would die.' But bearing in mind that the premises are not certain, they would remember that the conclusion was only to be held with a qualified assent. This they would express quite correctly, if the mere nature and not the degree of that assent is taken into account, by saying that 'he is pretty sure to die.' In this case the modality is rejected temporarily from the premises to be reintroduced into the conclusion.

---

[1] *Letters, Lectures and Reviews*, p. 61. Elsewhere in the review (p. 45) he gives what appears to me a somewhat different decision.

[2] It must be remembered that this is not one of the proportional propositions with which we have been concerned in previous chapters: it is meant that there are exactly 21 Ys, of which just 18 are X, not that on the average 18 out of *every* 21 may be so regarded.

It is obvious that such a process as this is of a very rough and imperfect kind. It does, in fact, omit from accurate consideration just the one point now under discussion. It takes no account of the varying shades of expression by which the degree of departure from perfect conviction is indicated, which is of course the very thing with which modality is intended to occupy itself. At best, therefore, it could only claim to be an extremely rude way of deciding questions, the accurate and scientific methods of treating which are demanded of us.

---

[1] I consider however, as I have said further on (p. 322), that the treatment in the older logics of Probable syllogisms, and Dialectic syllogisms, came to almost exactly the same thing as this, though they looked at the matter from a different point of view, and expressed it in very different language.

§ 8. In any employment of applied logic we have of course to go through such a process as that just mentioned. Outside of pure mathematics it can hardly ever be the case that the premises from which we reason are held with absolute conviction. Hence there must be a lapse from absolute conviction in the conclusion. But we reason on the hypothesis that the premises are true, and any trifling defection from certainty, of which we may be conscious, is mentally reserved as a qualification to the conclusion. But such considerations as these belong rather to ordinary applied logic; they amount to nothing more than a caution or hint to be borne in mind when the rules of the syllogism, or of induction, are applied in practice. When, however, we are treating of modality, the extent of the defection from full certainty is supposed to be sufficiently great for our language to indicate and appreciate it. What we then want is of course a scientific discussion of the principles in accordance with which this departure is to be measured and expressed, both in our premises and in our conclusion. Such a plan, therefore, as this for treating modality is just as much a banishment of it from the field of real logical enquiry, as if we had determined avowedly to reject it from consideration.

§ 9. Before proceeding to a discussion of the various ways in which modality may be treated by those who admit it into logic, something must be said to clear up a possible source of confusion in this part of the subject. In the cases with which we have hitherto been mostly concerned, in the earlier chapters of this work, the characteristic of modality (for in this chapter we may with propriety use this logical term) has generally been found in singular and particular propositions. It presented itself when we had to judge of individual cases from a knowledge of the average, and was an expression of the fact that the proposition relating to these individuals referred to a portion only of the whole class from which the average was taken. Given that of men of fifty-five, three out of five will die in the course of twenty years, we have had to do with modal propositions of the vague form, 'It is probable that *AB* (of that age) will die,' or of the more precise form, 'It is three to two that *AB* will die,' within the specified time. Here the modal proposition naturally presents itself in the form of a singular or particular proposition.

§ 10. But when we turn to ordinary logic we may find universal propositions spoken of as modal. This must clearly be the case with those which are termed necessary or impossible, but it may also be the case with the probable. We may meet with the form 'All *X* is probably *Y*.' Adopting the same explanation here as has been throughout adopted in analogous cases, we must say that what is meant by the modality in such a proposition is the proportional number of times in which the proposition would be correctly made. And in this there is, so far, no difficulty. The only difference is that whereas the explanation of the former, or individual, kind of modal was obtainable within the limits of the universal proposition which included it, the justification of the modality of a universal proposition has to be sought in a group or succession of other propositions. But this distinction is not at all fundamental.

It is quite true that universal propositions from their nature are much less likely than individual ones to be justified, in practice, by such appeal. But, as has been already frequently pointed out, we are not concerned with the way in which our propositions are practically obtained, nor with the way in which men might find it most natural to test them; but with that ultimate justification to which we appeal in the last resort, and which has been abundantly shown to be of a statistical character. When, therefore, we say that 'it is probable that all X is Y,' what we mean is, that in more than half the cases we come across we should be right in so judging, and in less than half the cases we should be wrong.

§ 11. It is at this step that the possible ambiguity is encountered. When we talk of the chance that All X is Y, we contemplate or imply the complementary chance that it is not so. Now this latter alternative is not free from ambiguity. It might happen, for instance, in the cases of failure, that no X is Y, or it might happen that some X, only, is not Y; for both of these suppositions contradict the original proposition, and are therefore instances of its failure. In practice, no doubt, we should have various recognized rules and inductions to fall back upon in order to decide between these alternatives, though, of course, the appeal to them would be in strictness extralogical. But the mere existence of such an ambiguity, and the fact that it can only be cleared up by appeal to the subject-matter, are in themselves no real difficulty in the application of the conception of modality to universal propositions as well as to individual ones.

§ 12. Having noticed some of the ways in which the introduction of modality into logic has been evaded or rejected, we must now enter into a brief account of its treatment by those who have more or less deliberately admitted its claims to acceptance.

The first enquiry will be, What opinions have been held as to the nature of modality? that is, Is it primarily an affection of the matter of the proposition, and, if not, what is it exactly? In reference to this enquiry it appears to me, as already remarked, that amongst the earlier logicians no such clear and consistent distinction between the subjective and objective views of logic as is now commonly maintained, can be detected. The result of this appears in their treatment of modality. This always had some reference to the subjective side of the proposition, viz. in this case to the nature or quantity of the belief with which it was entertained; but it is equally clear that this characteristic was not estimated at first hand, so to say, and in itself, but rather from a consideration of the matter determining what it should be. The Aristotelean division, for instance, is into the necessary, the contingent, the possible, and the impossible. This is clearly a division according to the matter almost entirely, for on the purely mental side the necessary and the impossible would be just the same; one implying full conviction of the truth of a proposition, and the other of that of its contradictory. So too, on the same side, it would not be easy to distinguish between the contingent and the possible. On the view in question, therefore, the modality of a proposition was determined by a reference to the nature of the subject-matter. In some propositions the nature of the subject-matter decided that the predicate was necessarily joined to the subject; in others that it was impossible that they should be joined; and so on.

§ 13. The artificial character of such a four-fold division will be too obvious to modern minds for it to be necessary to criticize it. A very slight study of nature and consequent appreciation of inductive evidence suffice to convince us that those uniformities upon which all connections of phenomena, whether called necessary or contingent, depend, demand extremely profound and extensive enquiry; that they admit of no such simple division into clearly marked groups; and that, therefore, the pure logician had better not meddle with them[1].

The following extract from Grote's *Aristotle* (Vol. I. p. 192) will serve to show the origin of this four-fold division, its conformity with the science of the day, and consequently its utter want of conformity with that of our own time:—"The distinction of Problematical and Necessary Propositions corresponds, in the mind of Aristotle, to that capital and characteristic doctrine of his Ontology and Physics, already touched on in this chapter. He thought, as we have seen, that in the vast circumferential region of the Kosmos, from the outer sidereal sphere down to the lunar sphere, celestial substance was a necessary existence and energy, sempiternal and uniform in its rotations and influence; and that through its beneficent influence, pervading the concavity between the lunar sphere and the terrestrial centre (which included the four elements with their compounds) there prevailed a regularizing tendency called Nature; modified, however, and partly counteracted by independent and irregular forces called Spontaneity and Chance, essentially unknowable and unpredictable. The irregular sequences thus named by Aristotle were the objective correlate of the Problematical Proposition in Logic. In these sublunary sequences, as to future time, *may or may not*, was all that could be attained, even by the highest knowledge; certainty, either of affirmation or negation, was out of the question. On the other hand, the necessary and uniform energies of the celestial substance, formed the objective correlate of the Necessary Proposition in Logic; this substance was not merely an existence, but an existence necessary and unchangeable...he considers the Problematical Proposition in Logic to be not purely subjective, as an expression of the speaker's ignorance, but something more, namely, to correlate with an objective essentially unknowable to all."

---

[1] It may be remarked that Whately (*Logic*, Bk. II. ch. II. § 2) speaks of necessary, impossible and contingent matter, without any apparent suspicion that they belong entirely to an obsolete point of view.

§ 14. Even after this philosophy began to pass away, the divisions of modality originally founded upon it might have proved, as De Morgan has remarked[1], of considerable service in mediæval times. As he says, people were much more frequently required to decide in one way or the other upon a single testimony, without there being a sufficiency of specific knowledge to test the statements made. The old logician "did not know but that any day of the week might bring from Cathay or Tartary an account of men who ran on four wheels of flesh and blood, or grew planted in the ground, like Polydorus in the Æneid, as well evidenced as a great many nearly as marvellous stories." Hence, in default of better inductions, it might have been convenient to make rough classifications of the facts which were and which were not to be accepted on testimony (the necessary, the impossible, &c.), and to employ these provisional inductions (which is all we should now regard them) as testing the stories which reached him. Propositions belonging to one of these classes were regarded as having an antecedent presumption against them so great as to prevail over almost any testimony worth taking account of, and so on.

§ 15. But this old four-fold division of modals continued to be accepted and perpetuated by the logicians long after all philosophical justification for it had passed away. So far as I have been able to ascertain, scarcely any logician of repute or popularity before Kant, was bold enough make any important change in the way of regarding the Even the Port-Royal Logic, founded as it is on Cartesianis repeats the traditional statements, though with extre brevity. This adherence to the old forms led, it need not remarked, to considerable inconsistency and confusion many cases. These forms were founded, as we have seen, an objective view of the province of logic, and this view w by no means rigidly carried out in many cases. In fact was beginning to be abandoned, to an extent and in directions which we have not opportunity here to discuss, before the influence of Kant was felt. Many, for instance, added to the list of the four, by including the true and the false; occasionally also the probable, the supposed, and the certain were added. This seems to show some tendency towards abandoning the objective for the subjective view, or at least indicates a hesitation between them.

---

[1] *Formal Logic*, p. 233.

§ 16. With Kant's view of modality almost every one is familiar. He divides judgments, under this head, into the apodeictic, the assertory, and the problematic. We shall have to say something about the number and mutual relations of these divisions presently; we are now only concerned with the general view which they carry out. In this respect it will be obvious at once what a complete change of position has been reached. The 'necessary' and the 'impossible' demanded an appeal to the matter of a proposition in order to recognize them; the 'apodeictic' and the 'assertory', on the

---

[1] The subject was sometimes altogether omitted, as by Wolf. He says a good deal however about probable propositions and syllogisms, and, like Leibnitz before him, looked forward to a "logica probabilium" as something new and desirable. I imagine that he had been influenced by the writers on Chances, as of the few who had already treated that subject nearly all the most important are referred to in one passage (*Philosophia Rationalis sive Logica*, § 593).

other hand, may be true of almost any matter, for they demand nothing but an appeal to our consciousness in order to distinguish between them. Moreover, the distinction between the assertory and the problematic is so entirely subjective and personal, that it may vary not only between one person and another, but in the case of the same person at different times. What one man knows to be true, another may happen to be in doubt about. The apodeictic judgment is one which we not only accept, but which we find ourselves unable to reverse in thought; the assertory is simply accepted; the problematic is one about which we feel in doubt.

This way of looking at the matter is the necessary outcome of the conceptualist or Kantian view of logic. It has been followed by many logicians, not only by those who may be called followers of Kant, but by almost all who have felt his influence. Ueberweg, for instance, who is altogether at issue with Kant on some fundamental points, adopts it.

§ 17. The next question to be discussed is, How many subdivisions of modality are to be recognized? Aristotle, and his followers generally, as we have seen, adopted a fourfold division. The exact relations of some of these to each other, especially the possible and the contingent, is an extremely obscure point, and one about which the commentators are by no means agreed. As, however, it seems tolerably clear that it was not consciously intended by the use of these four terms to exhibit a graduated scale of intensity of conviction, their correspondence with the province of modern probability is but slight, and the discussion of them, therefore, becomes rather a matter of special or antiquarian interest. De Morgan, indeed (*Formal Logic*, p. 232), says that the schoolmen understood by contingent more likely than not, and by possible less likely than not. I do not know on what authority this statement rests, but it credits them with a nearer approach to the modern views of probability than one would have expected, and decidedly nearer than that of most of their successors[1]. The general conclusion at which I have arrived, after a reasonable amount of investigation, is that there were two prevalent views on the subject. Some (e.g. Burgersdyck, Bk. I. ch. 32) admitted that there were at bottom only two kinds of modality; the contingent and the possible being equipollent, as also the necessary and the impossible, provided the one asserts and the other denies. This is the view to which those would naturally be led who looked mainly to the nature of the subject-matter. On the other hand, those who looked mainly at the form of expression, would be led by the analogy of the four forms of proposition, and the necessity that each of them should stand in definite opposition to each other, to insist upon a distinction between the four modals[2]. They, therefore, maintained (e.g. Crackanthorpe, Bk. III. ch. 11) that the contingent is that which now is but may not be, and the possible that which now is not but may be. A few appear to have made the distinction correspondent to that between the physically and the logically possible.

§ 18. When we get to the Kantian division we have reached much clearer ground. The meaning of each of these terms is quite explicit, and it is also beyond doubt that they have a more definite tendency in the direction of assigning a graduated scale of conviction. So long as they are regarded from a metaphysical rather than a logical standing point, there is much to be said in their favour. If we use introspection merely, confining ourselves to a study of the judgments themselves, to the exclusion of the grounds on which they rest, there certainly does seem a clear and well-marked distinction between those which we cannot even conceive to be reversed in thought; those which we could reverse, but which we accept as true; and those which we merely entertain as possible.

---

[1] I cannot find the slightest authority for the statement in the elaborate history of Logic by Prantl.

[2] "Hi quatuor modi magnam censeri solent analogiam habere cum quadruplici propositionum in quantitate et qualitate varietate" (Wallis's *Instit. Logic.* Bk. II. ch. 8).

Regarded, however, as a logical division, Kant's arrangement seems to me of very little service. For such logical purposes indeed, as we are now concerned with, it resolves itself into a two-fold division. The distinction between the apodeictic and the assertory will be admitted, I presume, even by those who accept the metaphysical or psychological theory upon which it rests, to be a difference which concerns, not the quantity of belief with which the judgments are entertained, but rather the violence which would have to be done to the mind by the attempt to upset them. Each is fully believed, but the one can, and the other cannot, be controverted. The belief with which an assertory judgment is entertained is full belief, else it would not differ from the problematic; and therefore in regard to the quantity of belief, as distinguished from the quality or character of it, there is no difference between it and the apodeictic. It is as though, to offer an illustration, the index had been already moved to the top of the scale in the assertory judgment, and all that was done to convert this into an apodeictic one, was to *clamp* it there. The only logical difference which then remains is that between problematic and assertory, the former comprehending all the judgments as to the truth of which we have any degree of doubt, and the latter those of which we have no doubt. The whole range of the former, therefore, with which Probability is appropriately occupied, is thrown undivided into a single compartment. We can hardly speak of a 'division' where one class includes everything up to the boundary line, and the other is confined to that boundary line. Practically, therefore, on this view, modality, as the mathematical student of Probability would expect to find it, as completely disappears as if it were intended to reject it.

§ 19. By less consistent and systematic thinkers, and by those in whom ingenuity was an over prominent feature, a variety of other arrangements have been accepted or proposed. There is, of course, some justification for such attempts in the laudable desire to bring our logical forms into better harmony with ordinary thought and language. In practice, as was pointed out in an earlier chapter, every one recognizes a great variety of modal forms, such as 'likely,' 'very likely,' 'almost certainly,' and so on almost without limit in each direction. It was doubtless supposed that, by neglecting to make use of technical equivalents for some of these forms, we should lose our logical control over certain possible kinds of inference, and so far fall short even of the precision of ordinary thought.

With regard to such additional forms, it appears to me that all those which have been introduced by writers who were uninfluenced by the Theory of Probability, have done little else than create additional confusion, as such writers do not attempt to marshal their terms in order, or to ascertain their mutual relations. Omitting, of course, forms obviously of material modality, we have already mentioned the true and the false; the probable, the supposed, and the certain. These subdivisions seem to have reached their climax in Occam (Prantl, III. 380), who held that a proposition might be modally affected by being 'vera, scita, falsa, ignota, scripta, prolata, concepta, credita, opinata, dubitata.'

§ 20. Since the growth of the science of Probability, logicians have had better opportunities of knowing what they had to aim at; and, though it cannot be said that their attempts have been really successful, these are at any rate a decided improvement upon those of their predecessors. Dr Thomson[1], for instance, gives a nine-fold division. He says that, arranging the degrees of modality in an ascending scale, we find that a judgment may be either possible, doubtful, probable, morally certain for the thinker himself, morally certain for a class or school, morally certain for all, physically certain with a limit, physically certain without limitation, and mathematically certain. Many other divisions might doubtless be mentioned, but, as every mathematician will recognize, the attempt to secure any general agreement in such a matter of arrangement is quite hopeless. It is here that the beneficial influence of the mathematical theory of Probability is to be gratefully acknowledged. As soon as this came to be studied it must have been perceived that in attempting to mark off clearly from one another certain gradations of belief, we should be seeking for breaches in a continuous magnitude. In the advance from a slight presumption to a strong presumption, and from that to moral certainty, we are making a gradual ascent, in the course of which there are no natural landing-places. The proof of this continuity need not be entered upon here, for the materials for it will have been gathered from almost every chapter of this work. The reader need merely be reminded that the grounds of our belief, in all cases which admit of number and measurement, are clearly seen to be of this description; and that therefore unless the belief itself is to be divorced from the grounds on which it rests, what thus holds as to their characteristics must hold also as to its own.

It follows, therefore, that modality in the old sense of the word, wherein an attempt was made to obtain certain natural divisions in the scale of conviction, must be finally abandoned. All that it endeavoured to do can now be done incomparably better by the theory of Probability, with its numerical scale, which admits of indefinite subdivision. None of the old systems of division can be regarded as a really natural one; those which admit but few divisions being found to leave the whole range of the probable in one unbroken class, and those which adopt many divisions lapsing into unavoidable vagueness and uncertainty.

---

[1] *Laws of Thought*, § 118.

§ 21. Corresponding to the distinction between pure and modal propositions, but even more complicated and unsatisfactory in its treatment, was that between pure and modal syllogisms. The thing discussed in the case of the latter was, of course, the effect produced upon the conclusion in respect of modality, by the modal affection of one or both premises. It is only when we reach such considerations as these that we are at all getting on to the ground appropriate to Probability; but it is obvious that very little could be done with such rude materials, and the inherent clumsiness and complication of the whole modal system come out very clearly here. It was in reference probably to this complication that some of the bitter sayings[1] of the schoolmen and others which have been recorded, were uttered.

Aristotle has given an intricate investigation of this subject, and his followers naturally were led along a similar track. It would be quite foreign to my purpose in the slight sketch in this chapter to attempt to give any account of these enquiries, even were I competent to do so; for, as has been pointed out, the connexion between the Aristotelean modals and the modern view of the nature of Probability, though real, is exceedingly slight. It need only be remarked that what was complicated enough with four modals to be taken account of, grows intricate beyond all endurance when such as the 'probable' and the 'true' and the 'false' have also to be assigned a place in the list. The following examples[1] will show the kind of discussions with which the logicians exercised themselves. 'Whether, with one premise certain, and the other probable, a certain conclusion may be inferred': 'Whether, from the impossible, the necessary can be inferred'; 'Whether, with one premise necessary and the other *de inesse*, the conclusion is necessary', and so on, endlessly.

---

[1] "Haud scio magis ne doctrinam modalium scholastici exercuerint, quam ea illos vexarit. Certe usque adeo sudatum hic fuit, ut dicterio locus sit datus; *De modalibus non gustabit asinus.*" Keckermann, *Syst. Log.* Bk. II. ch. 3.

§ 22. On the Kantian view of modality the discussion of such kinds of syllogisms becomes at once decidedly more simple (for here but three modes are recognized), and also somewhat more closely connected with strict Probability, (for the modes are more nearly of the nature of gradations of conviction). But, on the other hand, there is less justification for their introduction, as logicians might really be expected to know that what they are aiming to effect by their clumsy contrivances is the very thing which Probability can carry out to the highest desired degree of accuracy. The former methods are as coarse and inaccurate, compared with the latter, as were the roughest measurements of Babylonian night-watchers compared with the refined calculations of the modern astronomer. It is indeed only some of the general adherents of the Kantian Logic who enter upon any such

---

[1] *Smiglecii Disputationes*, Ingolstadt, 1618.

See also Prantl's *History of Logic*, (under *Occam* and *Buridan*) for accounts of the excessive complication which the subtlety of those learned schoolmen evolved out of such suitable materials.

hat 'very improbable' should be confined to those events which had odds of (say) more than 99 to 1 against them; and so on, with other similar expressions. There would, no doubt, be difficulties in the way, for in all applications of classification we have to surmount the twofold obstacles which lie in the way, first (to use Kant's words) of the faculty of making rules, and secondly of that of subsumption under rules. That s to say, even if we had agreed upon our classes, there would till be much doubt and dispute, in the case of things which id not readily lend themselves to be counted or measured, s to whether the odds were more or less than the assigned uantity.

It is true that when we know the odds for or against an vent, we can always state them explicitly without the neces-ity of first agreeing as to the usage of terms which shall mply them. But there would often be circumlocution and pedantry in so doing, and as long as modal terms are in practical use it would seem that there could be no harm, and might be great good, in arriving at some agreement as to the degree of probability which they should be generally understood to indicate. Bentham, as is well known, in despair of ever obtaining anything accurate out of the language of common life on this subject, was in favour of a direct appeal to the numerical standard. He proposed the employment, in judicial trials, of an instrument, graduated from 0 to 10, on which scale the witness was to be asked to indicate the degree of his belief of the facts to which he testified: similarly the judge might express the force with which he held his conclusion. The use of such a numerical scale, however, was to be optional only, not compulsory, as Bentham admitted that many persons might feel at a loss thus to measure the degree of their belief. (*Rationale of Judicial Evidence*, Bk. I., Ch. VI.)

§ 24. Throughout this chapter we have regarded the modals as the nearest counterpart to modern Probability which was afforded by the old systems of logic. The reason for so regarding them is, that they represented an attempt, rude as it was, to recognize and measure certain gradations in the degree of our conviction, and to examine the bearing of such considerations upon our logical inferences.

But although it is amongst the modals that the germs of the methods of Probability are thus to be sought; the subject-matter of our science, that is, the classes of objects with which it was concerned, are rather represented by another part of the old logic. This was the branch commonly called Dialectic, in the old sense of that term. Dialectic, according to Aristotle, seems to have been a sort of sister art to Rhetoric. It was concerned with syllogisms differing in no way from demonstrative syllogisms, except that their premises were probable instead of certain. Premises of this kind he termed topics (they went by the name of *loci* amongst the scholastic logicians), and the syllogisms which dealt with them enthymemes. They were said to start from 'signs and likelihoods' rather than from axioms[1].

§ 25. The terms in which such reasonings are commonly described sound very much like those applicable to Probability, as we now understand it. When we hear of likelihood, and of probable syllogisms, our first impression might be that the inferences involved would be of a similar character[1]. This, however, would be altogether erroneous. In the first place the province of this Dialectic was much too wide, for it covered in addition the whole field of what we should now term Scientific or Material Induction. The distinctive characteristic of the dialectic premises was their want of certainty, and of such uncertain premises Probability (as I have frequently insisted) takes account of one class only, Induction concerning itself with another class. Again, not the slightest attempt was made to enter upon the enquiry, *How* uncertain are the premises? It is only when this is attempted that we can be considered to enter upon the field of Probability, and it is because, after a rude fashion, the modals attempted to grapple with this problem, that we have regarded them as occupied with our special subject-matter.

---

[1] "The εἰκὸς and σημεῖον themselves are propositions; the former stating a *general probability*, the latter a fact, which is known to be an indication, more or less certain, of the truth of some further statement, whether of a single fact, or of a general belief. The former is a general proposition, nearly, though not quite, universal; as, 'most men who envy hate'; the latter is a *singular* proposition, which however is not regarded as a sign, except relatively to some other proposition, which it is supposed may be inferred from it." (Mansel's Aldrich; Appendix F, where an account will be found of the Aristotelean enthymeme, and dialectic syllogism. Also, of course, in Grote's Aristotle, *Topics*, and elsewhere.)

§ 26. Amongst the older logics with which I have made any acquaintance, that of Crackanthorpe gives the fullest discussion upon this subject. He divides his treatment of the syllogism into two parts, occupied respectively with the 'd monstrative' and the 'probable' syllogism. To the latter whole book is devoted. In this the nature and consequence of thirteen different 'loci' are investigated, though it is n very clear in what sense they can every one of them be regarded as being 'probable.'

---

[1] "Nam in hoc etiam differt demonstratio, seu demonstrativa argumentatio, à probabili, quia in illâ tam conclusio quam præmissæ necessariæ sunt; in probabili autem argumentatione sicut conclusio ut probabilis infertur ita præmissæ ut probabiles afferuntur" (Crackanthorpe, Bk. v., Ch. 1); almost the words with which De Morgan distinguishes between logic and probability in a passage already cited (see Ch. v. § 3).

Perhaps it was a development of some such view as this that Leibnitz looked forward to. "J'ai dit plus d'une fois qu'il faudrait une nouvelle espèce de Logique, qui traiteroit des degrés de Probabilité, puisqu' Aristote dans ses Topiques n'a rien moins fait que cela" (*Nouveaux essais*, Lib. IV. ch. XVI). It is possible, indeed, that he may have had in his mind more what we now understand by the mathematical theory of Probability, but in the infancy of a science it is of course hard to say whether any particular subject is definitely contemplated or not. Leibnitz (as Mr Todhunter has shown in his history) took the greatest interest in such chance problems as had yet been discussed.

It is doubtless true, that if the old logicians had bee in possession of such premises as modern Probability is concerned with, and had adhered to their own way of treatin them, they would have had to place them amongst such *loc* and thus to make the consideration of them a part of thei Dialectic. But inasmuch as there does not seem to have been the slightest attempt on their part to do more here than recognize the *fact* of the premises being probable; that is, since it was not attempted to *measure* their probability and that of the conclusion, I cannot but regard this part of Logic as having no relation to Probability as now conceived. It seems to me nothing more than one of the ways (described at the commencement of this chapter) by which the problem of Modality is not indeed rejected, but practically evaded.

§ 27. As Logic is not the only science which is directl and prominently occupied with questions about belief an evidence, so the difficulties which have arisen there hav been by no means unknown elsewhere. In respect of th modals, this seems to have been manifestly the case i Jurisprudence. Some remarks, therefore, may be conveniently made here upon this application of the subject, thoug of course with the brevity suitable on the part of a layma who has to touch upon professional topics.

Recall for a moment what are the essentials of modality These I understand to be the attempt to mark off from on another, without any resort to numerical notation, varyin degrees of conviction or belief, and to determine the conse quent effect of premises, thus affected, upon our conclusions. Moreover, as we cannot construct or retain a scale of any kind without employing a standard from and by which to measure it, the attainment and recognition of a standard of certainty, or of one of the other degrees of conviction, is inseparably involved in the same enquiry. In this sense of the term, modaldifficulties have certainly shown themselves in the department of Law. There have been similar attempts here, encountered by similar difficulties, to come to some definite agreement as to a scale of arrangement of the degrees of our assent. It is of course much more practicable to secure such agreement in the case of a special science, confined more or less to the experts, than in subjects into which all classes of outsiders have almost equal right of entry. The range of application under the former circumstances is narrower, and the professional experts have traditions amongst them by which the standards may be retained in considerable integrity. It does not appear, however, according to all accounts, as if any very striking success had been attained in this direction by the lawyers.

§ 28. The difficulty in its scientific, or strictly jurisprudential shape, seems to have shown itself principally in the attempt to arrange legal evidence into classes in respect of the degree of its cogency. This was the case in the Roman law, and in some of the continental systems of jurisprudence which took their rise from the Roman law. "The direct evidence of so many witnesses was *plena probatio*. Then came *minus plena probatio*, then *semiplenâ major* and *semiplenâ minor;* and by adding together a certain number of half-proofs—for instance, by the production of a tradesman's account-books, *plus* his supplementary oath—full proof might be made out. It was on this principle that torture was employed to obtain a confession. The confession was evidence suppletory to the circumstances which were held to justify its employment[1].”

According to Bentham[2], the corresponding scale in the English school was:—Positive proof, Violent presumption, Probable presumption, Light or Rash presumption. Though admitted by Blackstone and others, I understand that these divisions are not at all generally accepted at the present day.

§ 29. In the above we are reminded rather of modal syllogisms. The principal practical form in which the difficulty underlying the simple modal propositions presents itself, is in the attempt to obtain some criterion of judicial certainty. By ‘certainty’ here we mean, of course, not what the metaphysicians term apodeictic[3], for that can seldom or never be secured in practical affairs, but such a degree of conviction, short of this, as every reasonable person will feel to be sufficient for all his wants. Here again, one would think, the quest must appear, to accurate thinkers, an utterly hopeless one; an effort to discover natural breaks in a continuous magnitude. There cannot indeed be the least doubt that, amongst limited classes of keen and practised intellects, a standard of certainty, as of everything else, might be retained and handed down with considerable accuracy: this is possible in matters of taste and opinion where personal peculiarities of judgment are far more liable to cause disagreement and confusion. But then such a consensus is almost entirely an affair of tact and custom; whereas what is wanted in the case in question is some criterion to which the comparatively uninitiated may be able to appeal. The standard, therefore, must not merely be retained by recollection, but be generally recognizable by its characteristics. If such a criterion could be secured, its importance could hardly be overrated. But so far as one may judge from the speeches of counsel, the charges of judges, and the verdicts of juries, nothing really deserving the name is ever attained.

---

[1] Stephen's *General view of the Criminal Law of England*, p. 241.

[2] *Rationale of Judicial Evidence;* Bk. I. ch. VI.

[3] Though this is claimed by some Kantian logicians;—“Nie darf an einem angeblichen Verbrecher die gesetzliche Strafe vollzogen werden, bevor er nicht selbst das Verbrechen eingestanden. Denn wenn auch alle Zeugnisse und die übrigen Anzeigen wider ihn wären, so bleibt doch das Gegentheil immer möglich” (Krug, *Denklehre*, § 131).

§ 30. The nearest approach, perhaps, to a recognized standard is to be found in the frequent assurance that juries are not bound to convict only in case they have *no* doubt of the guilt of the accused; for the absolute exclusion of all doubt, the utter impossibility of suggesting any counter hypothesis which this assumes, is unattainable in human affairs. But, it is frequently said, they are to convict if they have no 'reasonable doubt,' no such doubt, that is, as would be 'a hindrance to acting in the important affairs of life.' As a caution against seeking after unattainable certainty, such advice may be very useful; but it need hardly be remarked that the certainty upon which we act in the important affairs of life is no fixed standard, but varies exceedingly according to the nature of those affairs. The greater the reward at stake, the greater the risk we are prepared to run, and conversely. Hardly any degree of certainty can exist, upon the security of which we should not be prepared to act under appropriate circumstances[1].

---

[1] As Mr C. J. Monro puts it: "Suppose that a man is suspected of murdering his daughter. Evidence which would not convict him before an ordinary jury might make a grand jury find a true bill; evidence which would not do this might make a coroner's jury bring in a verdict against him; evidence which would not do this would very often prevent a Chancery judge from appointing the man guardian to a ward of the court; evidence which would not affect the judge's mind might make

Some writers indeed altogether deny that any standard, in the common sense of the word, either is, or ought to be, aimed at in legal proceedings. For instance, Mr J. F. Stephen, in his work on English Criminal Law[1], after noticing and rejecting such standards as that last indicated, comes to the conclusion that the only standard recognized by our law is that which induces juries to convict:—"What is judicial proof? That which being permitted by law to be given in evidence, induces twelve men, chosen according to the Jury Act, to say that, having heard it, their minds are satisfied of the truth of the proposition which it affirms. They may be prejudiced, they may be timid, they may be rash, they may be ignorant; but the oath, the number, and the property qualification, are intended, as far as possible, to neutralize these disadvantages, and answer precisely to the conditions imposed upon standards of value or length." (p. 263.)

To admit this is much about the same thing as to abandon such a standard as unattainable. Evidence which induces a jury to convict may doubtless be a standard to me and others of what we ought to consider 'reasonably certain,' provided of course that the various juries are tolerably uniform in their conclusions. But it clearly cannot be proposed

---

a father think twice on his deathbed before he appointed the man guardian to *his* daughter."

[1] The portions of this work which treat of the nature of proof in general, and of judicial proof in particular, are well worth reading by every logical student. It appears to me, however, that the author goes much too far in the direction of regarding proof as subjective, that is as what *does* satisfy people, rather than as what *should* satisfy them. He compares the legislative standard of certainty with that of value; this latter is declared to be a certain weight of gold, irrespective of the rarity or commonness of that metal. So with certainty; if people grow more credulous the intrinsic value of the standard will vary.

as a standard to the juries themselves; if their decisions are to be consistent and uniform, they want some external indication to guide them. When a man is asking, *How* certain ought I to feel? to give such an answer as the above is, surely, merely telling him that he is to be as certain as he is. If, indeed, juries composed a close profession, they might, as was said above, retain a traditional standard. But being, as they are, a selection from the ordinary lay public, their own decisions in the past can hardly be held up to them as a direction what they are to do in future.

§ 31. It would appear therefore that we may fairly say that the English law, at any rate, definitely rejects the main assumption upon which the logical doctrine of modality and its legal counterpart are based: the assumption, namely, that different grades of conviction can be marked off from one another with sufficient accuracy for us to be able to refer individual cases to their corresponding classes. And that with regard to the collateral question of fixing a standard of certainty, it will go no further than pronouncing, or implying, that we are to be content with nothing short of, but need not go beyond, 'reasonable certainty.'

This is a statement of the standard, with which the logician and scientific man can easily quarrel; and they may with much reason maintain that it has not the slightest claim to accuracy, even if it have one to strict intelligibility. If a man wishes to know whether his present degree of certainty *is* reasonable, whither is he to appeal? He can scarcely compare his mental state with that which is experienced in 'the important affairs of life,' for these, as already remarked, would indicate no fixed value. At the same time, one cannot suppose that such an expression is destitute of all signification. People would not continue to use language, especially in matters of paramount importance and interest, without meaning something by it. We ar driven therefore to conclude that 'reasonable certainty' doe in a rude sort of way represent a traditional standard t which it is attempted to adhere. As already remarked, thi is perfectly practicable in the case of any class of professio men, and therefore not altogether impossible in the case those who are often and closely brought into connection wi such a class. Though it is hard to believe that any su expressions, when used for purposes of ordinary life, att at all near enough to any conventional standard to be wor discussion; yet in the special case of a jury, acting und the direct influence of a judge, it seems very possible th their deliberate assertion that they are 'fully convince may reach more nearly to a tolerably fixed standard tha ordinary outsiders would at first think likely.

§ 32. Are there then any means by which we coul ascertain what this standard is; in other words, by which we could determine what is the real worth, in respect of accuracy, of this 'reasonable certainty' which the juries are supposed to secure? In the absence of authoritative declarations upon the subject, the student of Logic and Probability would naturally resort to two means, with a momentary notice of which we will conclude this enquiry. In the first place he might appeal to direct statistics, as follows. Juries convict upon reasonable certainty; but (we might say, 'therefore') they sometimes go wrong, and their verdicts have to be set aside. In what proportion of cases are their decisions reversed upon legitimate authority? If this question could be answered, we should at once have a superior limit beyond which their certainty is found by experience not to be capable of being carried; assuming of course that the authority which corrects them is itself above suspicion of failure. It is not to be supposed that this can practically be done, but, if it could, we should be in the way of knowing what sort of a criterion of truth was afforded by the assurance of the jury that they were satisfied in their minds of the truth of their verdict; that is, we should have an indication of their standard of legal certainty.

A scarcely more hopeful means would be found in a reference to certain cases of legal 'presumptions.' A 'conclusive presumption' is defined as follows:—"Conclusive, or as they are elsewhere termed imperative or absolute presumptions of law, are rules determining the quantity of evidence requisite for the support of any particular averment which is not permitted to be overcome by any proof that the fact is otherwise[1]." A large number of such presumptions will be found described in the text-books, but they seem to refer to matters far too vague, for the most part, to admit of any reduction to statistical frequency of occurrence. It is indeed maintained that any assignment of degree of Probability is not their present object, but that they are simply meant to exclude the troublesome delays that would ensue if everything were considered open to doubt and question. Moreover, even if they did assign a degree of certainty this would rather be an indication of what legislators or judges thought reasonable than what was so considered by the juries themselves.

There are indeed presumptions as to the time after which a man, if not heard of, is supposed to be dead (capable of disproof, of course, by his reappearance). If this time varied with the age of the man in question, we should at once have some such standard as we desire, for a reference to the Life tables would fix his probable duration of life, and so determine indirectly the measure of probability which satisfied the law.

---

[1] Taylor on Evidence: the latter part of the extract does not seem very clear.

But this is not the case; the period chosen is entirely irrespective of age. The nearest case in point (and that scarcel amounts to anything) which I have been able to ascertain i that of the age after which it has been presumed that woman was incapable of bearing children. This was the ag of 53. The odds against such an event are, if Quetelet[1] an his authorities are to be trusted, roughly speaking, about 5000 to 1; that is to say, such an event does not occur above once in that number of times.

It need not be remarked that any such clues as these to the measure of judicial certainty are far too slight to be of any real value. They only deserve passing notice as a possible logical solution of the problem in question, or rather as an indication of the mode in which, in theory, such a solution would have to be sought, were the English law, on these subjects, a perfectly consistent scheme of scientific evidence. This is the mode in which one would, under those circumstances, attempt to extract from its proceedings an admission of the exact measure of that standard of certainty which it adopted, but which it declined openly to enunciate.

---

[1] *Physique Sociale*, Vol. I. p. 184. I go by his figures, not his diagram, which obviously represents a much smaller, in fact absurdly smaller, chance against the occurrence of such an event than the figures warrant.

CHAPTER XIII.

THE METHOD OF LEAST SQUARES; OR THE NATURE OF THE ADVANTAGE, IN THE CASE OF DISCORDANT RESULTS, OF TAKING THEIR MEAN.

§ 1. IN a former chapter (ch. II. §§ 3—10) we entered into some discussion upon the way in which erroneous results, or those which by a more or less justifiable analogy might be termed erroneous, are almost invariably found to group themselves ultimately about a mean. It was pointed out also, that there was a consilience of specific experience and direct reasoning to justify, to a very considerable extent, the common assumption that there is but one such way or law of grouping, known as 'the Law of Error,' or more precisely 'the Exponential Law of Error,' prevailing in all the various cases of the kind which come under our notice. It was at the same time mentioned that a third form of proof might be sought in the fact that one method of treating these errors (when we wish to elicit the truth out of them, in the way to be immediately explained), viz. that known as the Method of Least Squares, is employed, and satisfactorily employed, in all cases alike. This fact, it might be urged, would involve as a consequence, that there could be but one law of grouping in the phenomena which could thus be treated by one method, for different laws of grouping would demand different methods of treatment. This is the main point which we have now to examine.

In the course of this discussion I purposely use a varie of terms to express the elements which thus group themselv about a mean; calling them sometimes errors, sometimes o servations, and so on. Instrumental errors are, no doubt, o of the most important classes of these elements, and in the case, of course, 'error' is a perfectly appropriate term to em ploy. But the reader must bear in mind (as has alread repeatedly been pointed out) that the principle in accordanc with which things thus group themselves about a mean is i reality widely extensive in its application. In fact, we ma lay it down as a general rule that almost any kind of effor directed to a single aim, and thwarted by the requisite number of appropriate obstacles, will furnish similar phenomena Indeed the principle is wider even than this, for, as was shown, there are many similar classes of cases presented in nature, (of which the heights of a tolerably homogeneous group of men are a familiar instance,) in reference to which we cannot with propriety speak of efforts, or aims, or consequently, in strict propriety, of errors.

§ 2. In the investigations of the former chapters we were supposed to have the complete series (of observations or whatever else they might be) given to us as a whole, either actually or potentially; so that from these complete data we were able to select any individual elements or groups of elements at pleasure. By speaking of it as actually given, we mean that statistics will furnish, not necessarily the whole of it, but at any rate as large a portion of it as is needed for our purposes. By speaking of it as potentially given, we mean either that from a small observed portion of it the remainder may be inductively inferred, or, more frequently, that we are in possession of the law in accordance with which it is produced, thus getting the whole of it given in embryo. At any rate, we are supposed, in one way or another, to have the whole series within our reach, in the sense that we may know for certain with what relative degree of frequency any particular amount of error will tend in the long run to occur.

We have now to put ourselves into a very different position, what might be termed the converse position. We are now to suppose that some limited number of these errors (one or more, however many it may be) is set before us, and that we have no other clue given to us, except what may be elicited out of them, as to the particular series of which they form a part. They are simply given to us as so many 'errors,' and we are asked to draw the soundest conclusion, or rather to make the best conjecture we can, as to the position or magnitude of that mean from which they may be supposed to be straying. In other words, since this mean is the 'true' result, it is desired to know how we may best treat the given errors so as to elicit the truth out of them. Of course if we could observe enough of them they would in time begin to betray their mean pretty decisively, but we are seeking for some hints for their management when they are still comparatively few in number.

§ 3. Take the familiar example given by Herschel, of a man firing at a mark with a pistol. First, suppose that we know the point at which he aims. Given this fact, and supplementing it with our general knowledge of the Law of Error[1], we can readily infer the ultimate appearance of the target at which he has been practising, that is, the comparative density of his shot-marks about the centre and elsewhere. If asked to represent this density by shading the target, not only should we know that we must put the blackest spot at the centre, but we should know also how rapidly the shade was to lessen as we receded further from the centre. This is the position, in respect of our knowledge, assumed in the first paragraph of the last section.

---

[1] In strictness, to make our knowledge complete, we want something more specific than this. The exponential curve is rather a *class* of curves (to speak mathematically it involves one disposable constant), and to choose the appropriate one out of these we ought to have some information about the quality of the shooting. A good marksman, and a bad one, will both involuntarily scatter their shots according to the same general law; but the diminution of density, in receding from the centre, will, though thus obeying the same law, actually take place in one case

But now suppose that the spot which marks the centre were wiped out, so that we saw nothing but a bare surface spotted with his shot-holes, and we were asked to guess where the centre had been? If one hole only were shown, common sense would suggest our assuming (of course very precariously) that *that* was the centre. If two, common sense would suggest our taking the mid-point between them. But if three were given, common sense would be at a loss; it would suggest taking some intermediate point, but would have no rule by which to decide more exactly. This is the position, in respect of our knowledge, assumed in the second paragraph.

§ 4. The Method of Least Squares[1] is a general rule telling us how to proceed in the above, and in far more complicated cases. It has given rise to the profoundest mathematical investigations, into which, it need not be remarked, I have neither the power nor the wish to enter here. But it is hoped that, by the consideration of simple examples, the logical principles upon which it rests may be so far explained that the general reader shall clearly under-

---

with greater speed than in the other.

[1] In such simple cases as we are mostly occupied with in this chapter this method reduces itself to merely giving the arithmetical mean of the quantities in question; but I have preferred to speak of it by its well-known name.

stand *why* (that is, in what sense and to what degree) the first two conclusions of common sense may be justified, and how we are to proceed in the third and other cases.

I will briefly state the conclusions to which we shall be led, and then proceed to explain and justify them.

(1) In the first place, no method of treating the errors can be expected to give us the true result accurately, except by a lucky accident.

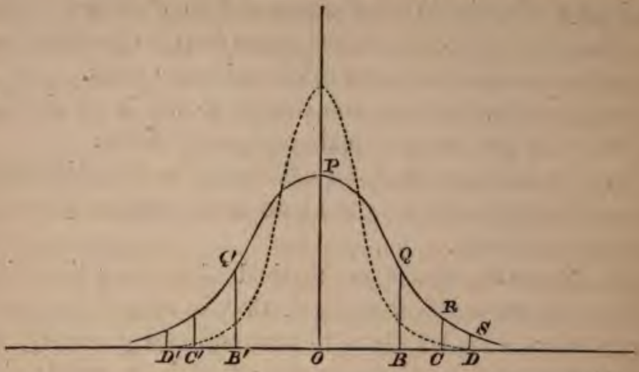
(2) Secondly, almost *any* method of treating them (any regular and symmetrical method, that is, subject to certain restrictions and explanations to be mentioned presently) will give us the truth in the long run; that is, it will tend to approximate indefinitely towards the truth, as the number of such errors is indefinitely multiplied.

(3) Thirdly, as a consequence of the foregoing, the only prerogative of one method over another must consist in the speed with which it approximates towards the truth. That is, if we make a good use of our observations we shall sooner get within any desired degree of the truth than if we make a bad use of them.

§ 5. (1) The first of the above statements will need but little explanation, and no justification, to any one who has grasped the principles of Probability; for it is characteristic of that science that no certain reliance can be placed upon the individual results obtained. Let us take an example to illustrate this. Suppose that by any kind of effort, whether of measurement or otherwise, we were endeavouring to obtain the point O along the line $D'OD$. We seldom or never hit it exactly[1], but are sometimes as much wrong in

---

[1] The mathematician will of course understand what is meant by 'hitting it exactly.' In strictness, if a point is to be regarded as less than any assignable magnitude, the chance of lighting on it will be small in the same degree; that is, there will be no finite chance of doing so. We must



one direction as D, sometimes as much so in the other as D'; sometimes we get as near as B or B'; occasionally, by a lucky hit, get it exactly[1], and so on. If we wish to represent the comparative frequency with which an error of any given magnitude occurs, we adopt the usual plan of drawing ordinates BQ, CR, DS, to stand for these relative frequencies. If BQ represent the number of times that the erroneous result B is obtained, CR represents that of the error OC, and so on. Hence the curve $Q'PQRS$ (whatever its nature may be) represents the 'law of frequency' of error in the case in question. With regard to the nature of this curve, no further assumptions are made than what are contained in the two conditions, (1) that there is no *bias* in the errors, that is, that they occur with equal frequency and magnitude in one direction as in the other, and (2) that great errors are less frequent than small ones. The former condition is expressed in Probability by saying that equal positive and negative errors are equally likely, and is illustrated

---

either regard the 'point' C as a very small length, or else consider that it is attained 'exactly' when we get within a certain very small distance of it.

[1] See Note on previous page.

graphically in the fact, that the curve in the figure is divided into two equal symmetrical parts by the line *OP*. The second is of course the familiar feature of most such attempts at an aim, and is illustrated in the fact that the curve, as it recedes from *PO*, tends to approach nearer to *OD*.

§ 6. Now since the particular errors *B*, *C*, *D*, &c. which have been set before us, are supposed to have been given at random out of the indefinitely extensive succession to which they belong, it is clear that there is nothing to prescribe the order in which they will occur. Hence it is exceedingly unlikely that, when we take in succession batches of two or more, they should just happen to fit in so precisely, as to balance each other's excess and defect. Of course they *may* do so: if it were anyhow predetermined that they should come out in equal and opposite pairs, first *B* and *B′* for instance, then *C* and *C′*, and so on, the middle point between each pair would always precisely hit that ultimate mean towards which we are painfully and doubtfully striving. Equally might this have been arranged with three or more such errors. If anything of this kind were really found to occur, the work of observation would indeed be rendered simple and accurate. But it need not be remarked that such happy adjustments as these are a mark either of design or else of occasional luck, and the slightest knowledge of the Theory of Combinations will remind us how exceptionally mere luck can be expected to furnish them.

The consequence of this absence of any such fixed order or arrangement in the occurrence of the successive errors is obvious. It follows that the precaution of not trusting to a single observation, but waiting for several for the sake of better security, great as we shall soon see its advantages to be in the long run, may nevertheless in individual cases happen to do mischief. Suppose that we had led off by obtaining *B*, we should as it happens have got a pretty fair result; but, wishing to make matters more sure, we try again and get *D*, say, the mid-point between which and *B* is further from the truth than *B* was. Hence in this case it would have turned out that our extra precaution was worse than wasted; we should have gone further and fared worse. This is of course unavoidable, for it springs out of that essential characteristic of the subject, the uncertainty and irregularity of the phenomena in detail. This uncertainty, therefore, being inherent in the errors themselves, exists independently of the number of them which we take into account, and of the particular method we adopt in treating them; that is, it cannot anyhow be entirely got rid of, though by appropriate methods (as will presently be shown) it can be diminished. Hence it is clear that no method of treating these errors can possibly insure accuracy in detail. Though we are likely to be better off with two results than with merely one, it is quite possible that even with the help of many we may be worse off.

§ 7. (2) The second proposition is closely connected with the first. Each of the members of any group selected out of the indefinite succession of errors having the characteristics distinctive of Probability, almost any result derived from such a group will exhibit the same characteristics essentially unaltered. All that we really do is to keep on reducing each plurality of uncertain single elements to an uncertain unity. Two or more 'errors' were given to us by observation. We take these, treat them according to the best process we can (the nature of some of these processes will be described presently), and substitute for them a single result; this single derivative result, or 'reduced observation' as it is sometimes termed, being still, let it be remembered, an 'error.' It cannot be divested, as we have already shown, of its inherent character of individual irregularity; but, on the other hand, it still retains, with some gain, its character of ultimate regularity. A combination of two or more errors in fact can scarcely fail, by any method of treatment, in giving us a new error of the same general character as the elements from which it was obtained.

For instance, the two main features of the regularity were (1) ultimate symmetry about the central or true result, and (2) increasing relative frequency as this centre was approached. A very little observation will show that both of these are retained. With regard to the symmetry; it is clear that, since in the case of the original errors, equal but opposite ones were on the average equally frequent, to every such set as (say) *B*, *C*, *D*, there will correspond a precisely similar set *B′*, *C′*, *D′*. If, therefore, we apply any rule, no matter what, to obtain some intermediate position from each of these sets, the results so obtained cannot fail to be equally distant from this mean, that is, they will in the long run be symmetrically situated. The two sets of positions *B*, *C*, *D* and *B′*, *C′*, *D′* differ in no respect whatever from one another, except in the fact that they are situated on opposite sides of the centre. Treat the two sets therefore according to the same rule, and the results derived from them will also differ in no other respect than this. We do not indeed know where this centre is, but this does not affect the fact of the symmetrical arrangement with respect to it. The other characteristic, of increasing frequency as the mean is approached, will better be deferred for a few minutes, but the reader will not find it difficult to believe that just as the original elements betrayed this characteristic, so do our derived results, and in fact (on which the whole value of such methods depends) with increasing rapidity.

§ 8. In the above reasoning we are merely taking account of the points B, C, D; B', C', D'; and the fact of their being symmetrically situated relative to the unknown centre O; we then say that the points given as derivative or 'reduced' positions will also be symmetrically situated. Of course if, instead of taking these points merely, we had taken their distances from some point X, it would no longer hold true that these *magnitudes* XB, XC, XD and XB', XC', XD', would, like the positions B, C, D, B', C', D', when similarly treated, yield results symmetrically situated about O. But it is not as magnitudes that they are to be regarded. If X is an origin arbitrarily chosen by us, we ought to confine our calculations (as is here done) to the positions of B, C, D, &c. relatively to one another. If, on the other hand, X is given in nature, that is, if the distances XB, XC, XD, &c. are actual magnitudes of space or time, it could hardl be the case (for reasons which need not here be dwelt upo) that our original supposition as to the symmetrical arrang ment of the points B, C, D, &c. about O, should have bee correct. In so far, therefore, as this supposition holds i reference to the original elements, in so far also will it hol in reference to the derivative elements, which is the point of the argument above.

§ 9. This will perhaps be better seen if we recur again for a moment to our former illustration of the target. First, suppose the man to have fired a great many shots aimed at the centre; we know well enough the general appearance which will be presented. But now suppose that a small number only of these shots, say three, are shown us, the remaining shots and the centre at which they were aimed being hidden from us. This is what may be called the inverse position, to guide us in which such a method as that of Least Squares is designed. We take those three, make the best conjecture we can (by the help of any one of a number of different rules which might be suggested) as to the position of the unseen centre; and then we continue to repeat this process again and again. The collection and arrangement of these conjectural positions of the centre will represent our series of derived or reduced results. It is asserted that their general arrangement will resemble that of the original shot-marks, being symmetrically aggregated, and with growing frequency, about the centre.

§ 10. I particularly desire to insist upon the fact that by thus combining our errors into groups of two or more, and then by some process reducing each such group to a single error, we simply get a new series of errors of precisely the same general type as the old series, with at most a somewhat different 'law of facility,' because any other view seems opposed to the fundamental principles of the science of Probability.

If it be objected[1] that in practice we should never treat these groups thus separately, but should prefer to wait till we had come to an end of them so as to combine all our separate elements into a single group, and that therefore there would be no opening for a *series* of the groups, I should refer to the arguments in a former chapter (Ch. v. §§ 19—24). The series with which Probability is concerned need not be actual, they may be potential merely, owing to one or a few only of the elements with which they are concerned being presented in actual experience, but our inferences still rest upon and assume an indefinite succession. However, this proviso is not wanted here. Whatever number of observations we may thus have taken, and reduced to one, there is no finality about the result so obtained. I may take up the instrument again, or somebody else may do so, and carry on the same process, and thus obtain more terms of the series, and so on indefinitely. This is an invariable postulate in Probability. If I toss a penny ten times, I may determine then to have done with it for ever, but this set of tosses is just as much an individual belonging to an indefinitely numerous succession as any single toss could be. When we talk of the probability of such and such a result in the ten tosses, we *mean* to refer to such a succession.

---

[1] As it very fairly may. For instance, the group of observations taken in the last Transit of Venus cannot be added to. But, as explained above, this does not touch the logical question.

§ 11. (3) But although the general type of the new arrangement (the arrangement, that is, of the resultant errors or reduced observations) will be similar to the old, the particular way in which they group themselves will be found to be different. It is upon this difference that everything turns, for if the new arrangement were in all respects the same as the old, we should have gained nothing by our trouble. The whole advantage, in fact, consists in this, that we secure results which cluster more thickly about the mean; of course when a sufficient number of each set are taken to institute a fair comparison.

Anything deserving the name of a proof that our reduced results are thus more closely aggregated about the centre can hardly be given here: but a few words of explanation will render such a state of things plausible, and indeed show how it takes place. It depends upon the doctrine of Combinations, by which it can be shown that central values can be got at in a great many more ways than extreme ones. The central ones can be obtained as the result of a balance of opposite errors, as well as the result of a concurrence of intermediate ones, whilst to secure an extreme result *all* the errors must be more or less extreme[1].

---

[1] This tendency to throw the results towards the centre is of course intensified by the fact that the elements from which they were obtained had

This is readily represented graphically. In the diagram on p. 338, if the old law of error be represented by the dark line, the new one will be represented by some such curve as that shown by the dotted line. It is contracted more towards the vertex, and shows a greater tendency to keep near to the central axis OP; expressive of the fact that out of a given number of results we shall find a larger proportion near the truth and a smaller proportion far from it.

§ 12. We have spoken about 'combining' two or more observations into one; something must now be said about the use of this expression, and about the various ways in which the process referred to by it may be performed. A certain number of elements are set before us: they may be observations; they are at any rate the results of some kind or other of effort at an aim. Of course if we had any means of knowing that one of them was decidedly the best, we should reject the others, and take that one alone; but since we cannot know this, we resolve to take them all into account. We then, by processes to be immediately discussed, combine them all together, that is, get a result which is dependent upon and influenced by them all, in other words, is a function of them. Having thus reduced them to one, we take this one as our substitute for any one or all together of the original plurality. But, as has been already insisted on, this final result possesses all the essential characteristics, with modifications merely, of the original single elements.

It has just been said that if we knew that one of our results was very much better than the others we should

already shown the same tendency, so that we are only magnifying a characteristic which they previously possessed. But the same tendency would come out as the consequence of taking reduced results even if the orginal errors had been all equally prevalent within certain limits, or even if extreme errors had been more frequent than intermediate ones.

simplify matters by rejecting all the others in its favour. It deserves notice here that in practice something of this kind is in most cases found to prevail. We cannot indeed detect beforehand any such superiority in one over others as to justify us in giving it the exclusive preference, but we may detect enough to make us rate it more highly. This leads to what is technically called 'weighting' observations. An astronomer, say, makes two observations, one in fine weather, or when his faculties are at their best, the other in worse weather, or when he is a trifle perturbed[1]. It would be absurd to put the two upon the same footing; accordingly, he reckons the former as if it had been made more frequently, say three times instead of once only. This does not in the slightest degree affect the principles discussed in the text. The only difference is that he is supposed to have made four observations instead of two, three of the number being the same. But the treatment of these four does not lead to any differences of principle as compared with what would have happened if he had confined himself to the two actually given.

§ 13. We must now enquire in what various modes the elements with which we are concerned may thus be combined and reduced. If we confine ourselves to two at a time, by taking them merely in pairs, there does not indeed seem any choice of treatment of general applicability. When two observations are given us, or two attempts of any kind at obtaining some position or estimating some magnitude, there is scarcely any other rational course to be adopted but that of taking their arithmetical mean[2], that is, the mid-point between them. It is quite true that however plausible and indeed unavoidable such a plan may seem, it must still (since we are in the region of Logic, not of Psychology) submit to justify itself, and such justification will be offered presently, if it is not considered that it has been already sufficiently implied.

---

[1] In practice, as I understand, observations are not thus directly estimated, but the value to be assigned to them is obtained rather by a method of successive approximation.

[2] That is, under the explanations and restrictions mentioned in § 8.

As soon, however, as we have three or more of these elements to take into consideration, a variety of different modes of treatment are open to us. We might, if we pleased, simply resolve to take the middlemost of the three, rejecting altogether the extreme ones. Or we might take the arithmetical mean of them, which is equivalent to choosing a point amongst them such that the sum of the squares of the errors measured from this point shall be a minimum. Each of these seems a very natural and convenient plan to adopt, in the absence of any authoritative declaration of the best plan. Or again, instead of making the sum of the squares a minimum, we might make the sum of any higher powers a minimum. The number of such possible ways of treatment open to us is indefinitely great.

In each of the methods indicated above, we are supposed to obtain some *intermediate* value, and therefore a result which though it may now and then be further from the truth than some one or two of the individual elements, will in the long run be nearer to it. If in our figure on p. 338, we were to draw a series of dotted lines to represent the respective laws of facility in each of these cases, we should find that they all possessed the common quality of being contracted towards *OP*, that is of expressing the fact that the resultant values diverge less, on an average, from the mean.

§ 14. Such general remarks as the above are readily intelligible, but, for anything more precise, appeal must be made to mathematics. In this respect, as the reader is very likely aware, a great amount of discussion has been carried on, involving the profoundest calculation, on the part of Laplace and others[1]. In the case of some writers, a good deal of this calculation is indirectly expended upon attempting to prove *à priori* what the nature of the original law of error, viz. that of the elements to which the appropriate method is to be applied, must be. There is, however, no doubt whatever that they are able satisfactorily to show that, *if* the exponential law of error is accepted, the superiority of the method of Least Squares follows at once. Moreover, on purely mathematical grounds, this method possesses immense practical advantages in the way of convenience of working; indeed it would hardly be saying too much to assert that it is the only method which admits of being put into practical use in any but the very simplest cases. When therefore we bear in mind that the exponential law of error can seldom be far from a correct expression of the facts, we have abundantly sufficient reasons to make employment of this method.

The reader may be reminded here that the Law of Error (whether of the exponential or any other form) and the Method of Least Squares are two perfectly distinct things. The former is a physical fact, in many cases, quite beyond our control; it is simply a scientific 'law,' that is a generalized statement as to the way in which certain events occur. It doubtless is the case that a certain form of this law is very commonly prevalent, but this is a matter to be established as other matters of fact are established. The Method of Least Squares (or any other corresponding method), on the other hand, is no law at all in this sense;

---

[1] There is an excellent account of these mathematical investigations in a paper by Mr J. W. L. Glaisher, published in the *Transactions of the Royal Astronomical Society*, Vol. XXXIX.

it is rather a rule or precept for our guidance. The Law states, in any given case, how the errors tend to occur; the Method informs us how to treat the errors when any number of them have occurred and are set before us.

§ 15. The fact indeed is, that there is nothing to prevent us from using the Method of Least Squares in any case we please, even though we happened to know or suspect that the errors were not arranged upon any plan at all resembling the exponential law. Any method of combination (of the general description mentioned in § 7) must, as it appears to me, give results in the long run better than what would be obtained by trusting to the single observations. Any such method must, indeed, in the long run tend to give us the truth, the worst that can be said against it being that some other method would do the same with less frequent risk of the larger errors. We can never be *wrong* in using the Method of Least Squares, and the drawback arising from its not being necessarily the best may be more than counterbalanced by the greater practical conveniences attending its use.

In thus saying that we shall in any case tend to approximate towards the 'truth,' some correction and explanation is needed. It is assumed, in so saying, that we are speaking of efforts consciously aimed at some object. Now it is quite conceivable, though almost impracticable, that, within certain limits, *all* deflections should be equally likely; it is also conceivable, and by no means impracticable, that (again within certain limits) great deflections should be more likely than small ones[1]. In these cases, since we really wanted to attain the centre, any method which helps us in so doing (as the plan of taking combinations of two or more does) is to be recommended. We are thus helped in bringing out, by groups of attempts, a tendency which we had somewhat failed in bringing out by our single attempts.

---

[1] M. Bravais, for instance, has shown how this might easily be the case (see a letter from him to Quetelet, published in the Appendix to the *Letters on Probability*, and in that to the first volume of the *Physique Sociale*).

But a similar 'double aim,' as regards the results, may be detected in cases where there was no single aim in intention. If so, to combine our observations, and thus to tend towards a central value, would not in any sense be tending towards the truth. This may often be the case in groups presented by nature, in which, if we are to seek her purpose in her deeds, we must recognize these two aims. Take, for instance, a large number of examples from two very heterogeneous populations; we should find amongst them, to use the language explained, but criticized, in a former chapter, 'two means,' so that if we lose sight of this, and merely tend towards the arithmetical average of them all, we are losing sight of some of the facts.

§ 16. If then we are asked, generally, why instead of adhering to a single observation, we take several, and by some mode of treatment reduce them to a single one; or, more particularly, why out of all the possible modes of thus treating them we should select some one as preferable; the only answer is that such methods in general give a 'more probable value,' and that one of them in particular gives a 'most probable value.' The meaning of such an answer, when drawn out, is as follows: By no method of treating these erroneous results can we make certain of attaining the truth (for no jugglery can thus elicit the certain out of the uncertain); and by no method even can we make sure of altogether avoiding errors as bad as any to which the original observations were liable, but we can succeed in making the bad errors much less frequent in proportion.

All reasonable methods, founded upon combinations of two or more observations, thus diminish the relative numbers of the larger errors, and that method is the best which diminishes them the most. To secure that out of a given number of trials we shall succeed pretty closely in a much greater number of cases, and that, when we do miss, the slight misses shall be comparatively much more frequent and the large misses much less frequent, is all that we can attain; but, for practical purposes, to attain this is to attain a great deal. The final results therefore that we secure are not probable in any other sense than that in which the original elements were probable; they merely possess this quality in a higher degree. It is precisely as if they were a corresponding set of single direct observations only made with a better instrument.

§ 17. We may now see how very slight is the weight of proof which can be furnished in favour of the universality of the Exponential Law of Error as drawn from the fact that the Method of Least Squares is successfully used in Astronomy and elsewhere. To assert that this Method leads to truth in the long run, is not enough, for (under certain restrictions already pointed out) *any* corresponding method will do this, and will do it with any law of error. It is true that mathematical reasoning will show that on the assumption of the Exponential Law the Method of Least Squares gives the most probable result, but it does not show that this would not, to a merely somewhat less extent, also be the case on the assumption of any other Law of Error. There may be many laws of error besides the particular Exponential one, and many methods besides that of Least Squares, and any such methods may be employed upon the results of any such laws, with at most (as it seems to me) a balance of advantage or disadvantage.

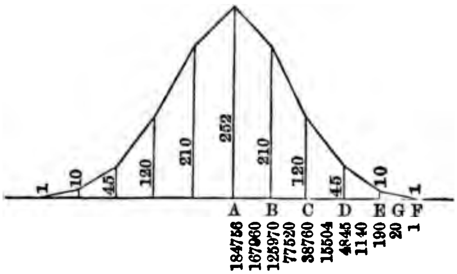
We may sum up then by saying that the use of this Method is abundantly justified by our being able to prove that it is the best method upon a reasonably probable supposition as to the nature of the law of error; a good method, at any rate, if not the best, upon most other suppositions; and in practice exceedingly easy and convenient to work.

§ 18. Perhaps the best way of bringing home the above results to those who are unfamiliar with the general notation of mathematics, will be found by taking a numerical example of a series of elements formed according to a law of error. We can then proceed to work out arithmetically the results of combining two or more of these errors together so as to get a new series, not contenting ourselves, as above, with the general character merely of the new law of error, but actually calculating what it is in the given case. For the sake of simplicity we will not take a series with a very large number of terms in it, but it will be well to have enough of them to secure that our law of error shall roughly approximate in its form to the standard or exponential law.

For this purpose the law of error or divergence given by supposing our effort to be affected by ten causes, each of which produces an error of a unit, but which error is equally likely to be positive and negative (or, as it might perhaps be expressed, 'ten equal and indifferently additive and subtractive causes') will suffice. This is the lowest number formed according to the Binomial law, which will furnish to the eye a fair indication of the limiting or Exponential law[1]. The whole cycle of possible cases here is 2^{10} or 1024; that is, this is the number required to exhibit not only all the cases which can occur (for there are but eleven really distinct cases), but also the relative frequency with which each of these cases occurs in the long run. Of this total, 252 will be situated at the mean, representing the true result, or that given when five of the causes of disturbance just neutralize the other five. Again, 210 will be at what we will call one unit's distance from the mean, and so on; until at the extreme distance of five places from the mean we get but one result, since in only one case out of the 1024 will all the causes combine together in the same direction. This cycle of 1024 efforts is therefore a fair representation of the distribution of an infinite number of such efforts. A graphical representation of the arrangement is given here.

---

[1] See, for the explanation of this, and of the graphical method of illustrating it, the note on p. 28.



§ 19. This representing a complete cycle of single observations or efforts, what will be the number and arrangement in the corresponding cycle of combined or reduced observations, say of two together? With regard to the number we must bear in mind that this is not a case of the combinations of things which cannot be repeated; for any given error, say the extreme one at *F*, can obviously be repeated twice running. Such a repetition would be a piece of very bad luck no doubt, but being possible it must have its place in the cycle. Now the possible number of ways of combining 1024 things two together, where the same thing may be repeated twice running, is 1024 × 1024 or 1048576.

This then is the number in a complete cycle of the resul taken two and two together.

§ 20. So much for their number; now for their arrangement or distribution. What we have to ascertain is, firstl how many times each possible pair of observations will prese itself; and, secondly, where the new results, obtained fro the combination of each pair, are to be placed. With regar to the first of these enquiries;—it will be readily seen tha on one occasion we shall have F repeated twice; on 20 occasions we shall have F combined with E (for F coming first w may have it followed by any one of the 10 at E, or an one of these may be followed by F); E can be repeated i 10 × 10, or 100 ways, and so on.

Now for the position of each of these reduced observations, the relative frequency of whose component elements has thus been pointed out. This is easy to determine, for when we take two errors there is (as was seen) scarcely any other mode of treatment than that of selecting the mid-point between them; this mid-point of course becoming identical with each of them when the two happen to coincide. It will be seen therefore that F will recur once on the new arrangement, viz. by its being repeated twice on the old one. G, midway between E and F, will be given 20 times. E, on our new arrangement, can be got at in two ways, viz. by its being repeated twice (which will happen 100 times), and by its being obtained as the mid-point between D and F (which will happen 90 times). Hence E will occur 190 times altogether.

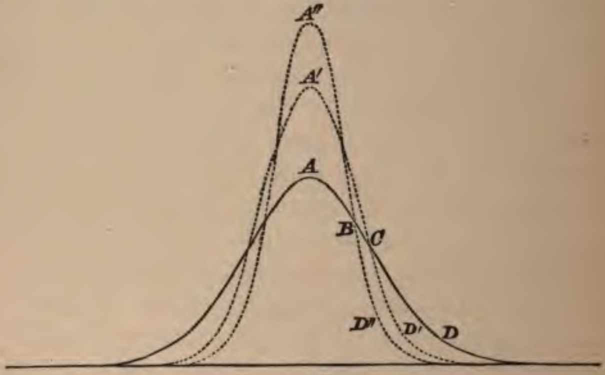
The reader who chooses to take the trouble may work out the frequency of all possible occurrences in this way, and if the object were simply to illustrate the principle in accordance with which they occur, this might be the best way of proceeding. But as he may soon be able to observe, and as the mathematician would at once be able to prove, the new 'law of facility of error' can be got at more quickly deductively, viz. by taking the successive terms of the expansion of $(1+1)^{20}$. They are given, below the line, in the figure on p. 353.

§ 21. There are two apparent obstacles to any direct comparison between the distribution of the old set of simple observations, and the new set of combined or reduced ones. In the first place, the number of the latter is much greater. This, however, is readily met by reducing them both to the same scale, that is by making the same total number of each. In the second place, half of the new positions have no representatives amongst the old, viz. those which occur midway between *F* and *E*, *E* and *D*, and so on. This can be met by the usual plan of filling in such gaps by estimating what would have been the number at the missing points, on the same scale, had they been occupied. Draw a curve through the vertices of the ordinates at *A*, *B*, *C*, &c., and the lengths of the ordinates at the intermediate points will very fairly represent the corresponding frequency of the errors of those magnitudes respectively. When the gaps are thus filled up, and the numbers thus reduced to the same scale, we have a perfectly fair basis of comparison. (See figure on next page.)

Similarly we might proceed to group or 'reduce' three observations, or any greater number. The number of possible groupings naturally becomes very much larger, being $(1024)^3$ when they are taken three together. As soon as we get to three or more observations, we have (as already pointed out) a variety of possible modes of treatment or reduction, of which that of taking the arithmetical mean is but one.

§ 22. The following figure is intended to illustrate the nature of the advantage secured by thus taking the arithmetical mean of several observations.

The curve *ABCD* represents the arrangement of a given number of 'errors' supposed to be disposed according to the binomial law already mentioned. *A′CD′* represents the similar arrangement of the same number when given not as simple errors, but as reductions from pairs of errors. *A″BD″*, again, represents the similar arrangement obtained as reductions from errors taken three together. They are drawn as carefully to scale as the small size of the figure permits.



A glance at the above figure will explain to the reader better perhaps than any verbal description could do, the full significance of the statement that the result of combining two or more observations together and taking the mean of them, instead of trusting to the single ones, is to make large errors comparatively much more scarce. It shows also how much better a result is likely to be which depends on three observations than one which depends on two only, and so on with larger groups.

§ 23. In the foregoing discussion we have taken the 'binomial law of error' as our basis, because, although not the most frequent one in nature, it is an approximation to one which certainly is very frequent. But the reader must clearly bear in mind that that useful tendency exhibited by the reductions from combinations of two or more errors, to throw the results more towards the mean, that is to mass them more about the correct value, is quite independent of their original arrangement. In this case they were already massed there to some degree (by the assumption of their being grouped according to the binomial law), and therefore the consequence of our processes of combining the observations in twos or threes together, merely increases a tendency already exhibited. But if all errors, within certain limits, had been equally prevalent, or even if extreme ones had been more prevalent than small ones, we should have still found the same general result, viz. that it is better to take several together rather than to trust to a single one, for by so doing we diminish the liability of encountering the larger errors. This tendency is a consequence of the mathematical properties of combinations, adopting of course the usual physical assumptions necessary to extract matters of fact out of conditions of number or space; the assumption in this case being that the various combinations are to occur with that degree of frequency which is implied by the causes of the errors being what is called 'independent.' In conclusion, then, if we are asked why we subject our observations to the rule of Least Squares, or any other similar rule, all that we can say is, that by so doing we hit the truth oftener than we do by trusting to single observations, and that the character of our errors, where we do err, is improved by the larger ones being made less frequent in proportion and the smaller ones more frequent. All that we ask of the mathematicians is that they shall inform us which of the various possible methods does this most effectually. But we must not overlook the fact that all reasonable methods afford good results, and that the superiority of one method over another is but small in comparison with their common superiority over single observations.

CHAPTER XIV.

FALLACIES.

§ 1. In works on Logic a chapter is generally devoted to the discussion of Fallacies, that is, to the description and classification of the different ways in which the rules of Logic may be transgressed. The analogy of Probability to Logic is sufficiently close to make it advisable to adopt the same plan here. In describing his own opinions an author is, of course, perpetually obliged to describe and criticize those of others which he considers erroneous. But some of the most widely spread errors find no supporters worth mentioning, and exist only in vague popular misapprehension. It will be found the best arrangement, therefore, at the risk of occasional repetition, to collect a few of the errors that occur most frequently, and as far as possible to trace them to their sources; but it will hardly be worth the trouble to attempt any regular system of arrangement and classification. We shall mainly confine ourselves, in accordance with the special province of this work, to problems which involve questions of logical interest, or to those which refer to the application of Probability to moral and social science. We shall avoid the discussion of isolated problems in games of chance and skill except when some error of principle seems to be involved in them.

§ 2. (I.) One of the most fertile sources of error confusion upon the subject has been already several ti alluded to, and in part discussed in a previous chap This consists in choosing the class to which to refer event, and therefore judging of the rarity of the event the consequent improbability of foretelling it, *after it happened*, and then transferring the impressions we ex rience to a supposed contemplation of the event beforeha The process in itself is perfectly legitimate (however un cessary it may be), since time does not in strictness en at all into questions of Probability. No error theref need arise in this way, if we were careful as to the c which we thus selected; but such carefulness is often n lected.

An illustration may serve to make this plain. A n once pointed to a small target chalked upon a door, target having a bullet hole through the centre of it, surprised some spectators by declaring that he had fired t shot from an old fowling-piece at a distance of a hund yards. His statement was true enough, but he suppres a rather important fact. The shot had really been aimed a general way at the barn-door, and had hit it; the tar was afterwards chalked round the spot where the bu struck. A deception analogous to this is, I think, of practised unconsciously in other matters. We judge events on a similar principle, feeling and expressing surp in an equally unreasonable way, and deciding as to th occurrence on grounds which are really merely a subsequ adjunct of our own. Butler's remarks about the story Cæsar, discussed already in the eleventh chapter, are this character. He selects a series of events from histo and then imagines a person guessing them correctly w at the time had not the history before him. As I ha already pointed out, it is one thing to be unlikely to guess an event rightly without specific evidence; it is another and very different thing to appreciate the truth of a story which is founded partly or entirely upon evidence. But it is a great mistake to transfer to one of these ways of viewing the matter the mental impressions which properly belong to the other. It is like drawing the target afterwards, and then being surprised to find that the shot lies in the centre of it.

§ 3. One aspect of this fallacy has been already discussed, but it will serve to clear up difficulties which are often felt upon the subject if we reexamine the question under a somewhat more general form.

In the class of examples under discussion we are generally presented with an individual which is not indeed definitely referred to a class, but in regard to which we have no great difficulty in choosing the appropriate class. Now suppose we were contemplating such an event as the throwing of sixes with a pair of dice four times running. Such a throw would be termed a very unlikely event, and the odds against its happening would be said to be $36 \times 36 \times 36 \times 36 - 1$ to 1 or 1679615 to 1. The meaning of these phrases, as has been abundantly pointed out, is simply that the event in question occurs very rarely; that, stated with numerical accuracy, it occurs once in 1679616 times.

§ 4. But now let us make the assumption that the throw has actually occurred; let us put ourselves into the position of contemplating sixes four times running, when it is supposed to be known or reported that this throw has happened. The same phrase, namely that the event is a very unlikely one, will often be used in relation to it, but we shall find that this phrase may be employed to indicate, on one occasion or another, extremely different meanings. Properly speaking an appeal to Probability is not very appropriate then; the throw in question being supposed to have happened, things are in a stage in relation to that throw in which all inferences from the science of Probability are, in the majority of cases, by rights superseded. The event is known, and therefore we need not now judge by means of statistics as to what might have been expected to occur, had it not been known. When however as is often the case, Probability is appealed to in reference to such a throw (as of course it may be, if we like to resort to its rules of inference), we shall find that two or three quite distinct meanings are, or may be, introduced.

§ 5. (1) There is, firstly, the most correct meaning The event, it is true, has happened, and we know what it is and therefore, as just stated, we have not really any occasion to resort to the rules of Probability; but we can nevertheless conceive ourselves as being in the position of a person who does not know, and who has only Probability to appeal to. By calling the chances 1679615 to 1 against the throw we then mean to state the fact, that inasmuch as such a throw occurs only once in 1679616 times, our guess, were we to guess, would be correct only once in the same number of times; provided, that is, that it is a fair guess, based simply on these statistical grounds.

§ 6. (2) But there is a second and very different conception sometimes introduced, especially when the event in question is supposed to be known, not as above by the evidence of our experience, but by the report of a witness. We may then mean by the 'chances against the event' (as was pointed out in Chapter XI.) not the proportional number of times we should be right in guessing the event, but the proportional number of times the witness will be right in reporting it. The bases of our inference are here shifted on to altogether new ground. In the former case the statistics were the throws and their respective frequency, now they are the witnesses' statements and their respective truthfulness.

§ 7. (3) But there is yet another meaning sometimes intended to be conveyed when persons talk of the chances against such an event as the throw in question. They may mean — not, Here is an event, how often should I have guessed it?—nor, Here is a report, how often will it be correct? — but something entirely different from either, namely, Here is an event, how often will it be found to be produced by some one particular kind of cause?

This meaning will often be found to introduce itself in the case of coincidences. When, for example, a man hears of dice giving as above the same throw several times running, and speaks of this as very extraordinary, we shall often find that he is not merely thinking of the improbability of his guess being right, or of the report being true, but, that along with this, he is introducing the question of the throw having been produced by fair dice. There is, of course, no reason whatever why such a question as this should not also be referred to Probability, provided always that we could find the appropriate statistics by which to judge. These statistics would be composed, not of throws of the particular dice, nor of reports of the particular witness, but of the occasions on which such a throw as the one in question respectively had, and had not, been produced fairly. The objection to entering upon this view of the question would be that no such statistics are obtainable, and that if they were, we should prefer to form our opinion (on principles to be described in Chapter XVI.) from the special circumstances of the case rather than from an appeal to the average.

§ 8. The reader will easily be able to supply any number of examples in illustration of the distinctions just given; we will briefly examine but one. I hide a banknote in a certain book in a large library, and leave the room. A person tells me that, after I went out, a stranger came in, walked straight up to that particular book, and took it away with him. Many people on hearing this account would reply, How extremely improbable! On analysing the phrase, I think we shall find that certainly two, and possibly all three, of the above meanings are involved in this exclamation. (1) What may be meant is this,—Assuming that the report is true, and the stranger innocent, a rare event has occurred. Many books might have been thus taken without that particular one being selected. I should not therefore have expected the event, and when it has happened I am surprised. Now a man has a perfect right to be surprised if he pleases, but he has no logical right (so long as we confine ourselves to this view) to make his surprise a ground for disbelieving the event. To do this is to fall into the fallacy described at the commencement of this chapter. The fact of my not having been likely to have guessed a thing beforehand is no reason in itself for doubting it when I am informed of it. (2) Or I may stop short of the events reported, and apply the rules of Probability to the report itself. If so, what I mean is that such a story as this now before me is of a kind very generally false, and that I cannot therefore attach much credit to it now. (3) Or I may accept the truth of the report, but doubt the fact of the stranger having taken the book at random. If so, what I mean is, that of men who take books in the way described, only a small proportion will be found to have taken them really at random; the majority will do so because they had by some means ascertained, or come to suspect, what there was inside the book.

§ 9. Each of the above three meanings is a possible and a legitimate meaning. The only requisite is that we should be very careful to ascertain which of them is present to the mind, so as to select the appropriate statistics. The first makes in itself the most legitimate use of Probability; the only drawback being that at the precise time in question the functions of Probability are superseded by the event being otherwise known. The second or third, therefore, or perhaps some combination of the two, is the more likely meaning to be present to the mind, for in these cases Probability, if it could be practically made use of, would, at the time in question, be a means of drawing really important inferences. The drawbacks here, are the impossibility of finding such statistics, and the extreme disturbing influence upon these statistics of the circumstances of the special case. Although, therefore, we frequently draw conclusions in such a case, on the principles of our science, we cannot do this with any such approach to accuracy as would justify us in accepting any numerical results.

§ 10. (II.) The fallacy described at the commencement of this chapter arose from determining to judge of an observed or reported event by the rules of Probability, but employing an entirely wrong set of statistics in the process of judging. Another fallacy, closely connected with this, arises from the practice of taking some only of the characteristics of such an event, and arbitrarily confining to these the appeal to Probability. An example may serve to make this plain. I toss up twelve pence, and find that eleven of them give heads. Many persons on witnessing such an occurrence would experience a feeling which they would express by the remark, How near that was to getting all heads! And if any thing very important were staked on the throw they would be much excited at the occurrence. But in what sense were we near to twelve? The number eleven of cours is nearer to twelve than ten or nine are, but there is surel something more than this in the person's mind at the moment. There is a not uncommon error, I apprehend, whic consists in unconsciously regarding the eleven heads as thing which is already somehow secured, so that one migh as it were keep them, and then take our chance for securin the remaining one. The eleven are mentally set aside looked upon as certain (for they have already happened), an we then introduce the notion of chance merely for th twelfth. But this twelfth, having also happened, has n better claim to such a distinction than any of the others If we will introduce the notion of chance in the case of th one that gave tail we must do the same in the case of all the others as well. In other words, if the tosser be dissatisfied at the appearance of the one tail, and wish to cancel it and try his luck again, he must toss up the whole lot of pence again fairly together. In this case, of course, so far from his having a better prospect for the next throw he may think himself in very good luck if he makes again as good a throw as the one he rejected. What he is doing is confounding this case with that in which the throws are really *successive.* If eleven heads have been tossed up in turn, we are of course within an even chance of getting a twelfth; but the circumstances are quite different in the instance proposed.

§ 11. In the above example the error is so transparent that a very slight amount of reflection will enable any one to see through it. But in forming a judgment upon matters of greater complexity than dice and pence, especially in the case of what are called 'narrow escapes,' a mistake of an analogous kind is, I apprehend, far from uncommon. A person, for example, who has just experienced a narrow escape will often be filled with surprise and anxiety amounting almost to terror. The event being past, these feelings are, at the time, in strictness inappropriate. If, as is quite possible, they are merely instinctive, or the result of association, they do not fall within the province of any kind of Logic. If, however, as seems to me far more likely, they partially arise from a supposed transference of ourselves into that point of past time at which the event was just about to happen, and the production by imagination of the feelings we should then expect to experience, this process partakes of the nature of an inference, and can be right or wrong. In other words, the alarm may be proportionate or disproportionate to the amount of danger that might fairly have been reckoned upon in such a hypothetical anticipation. If we attend to the remarks frequently uttered by people on such occasions, we shall find, I think, that they do distinctly consider that their feelings admit of justification; if so, I do not perceive by what other process they can be justified than by that which has been just described. If the supposed transfer were completely carried out, there would be no fallacy; but it is often very incompletely done, some of the component parts of the event being supposed to be determined or 'arranged' (to use a sporting phrase) in the form in which we now know that they actually have happened, and only the remaining ones being fairly contemplated as future chances by Probability.

A man, for example, is out with a friend, whose rifle goes off by accident, and the bullet passes through his hat. He trembles with anxiety at thinking what might have happened, and perhaps remarks, 'How very near I was to being killed!' Now we may safely assume that he means something more than that a shot passed very close to him; such an event might have been produced purposely and cautiously by his friend, and in that case his feelings would have been totally different. He has now some vague id that, as he would probably say, 'his chance of being kill then was very great.' His surprise and terror may be great part physical and instinctive, arising simply from th knowledge that the shot had passed very near him. B his mental state may be analysed, and we shall then mo likely find, at bottom, a fallacy of the kind described above To speak or think of chance in connection with the inciden or to go back to what might have been, is to refer the particular incident to a class of incidents of a similar character, an then to consider the comparative frequency with which th contemplated result ensues. Now the series which we may suppose to be most naturally selected in this case is one composed of shooting excursions with his friend; up to this point the proceedings are assumed to be designed, beyond it only, in the subsequent event, was there accident. Once in a million times perhaps on such occasions the gun will go off accidentally; one in a thousand only of those discharges will be directed near his friend's head. If we will make the accident a matter of Probability, we ought by rights in this way (to adopt the language of the first example), to 'toss up again' fairly. But we do not do this; we seem to assume for certain that the shot goes within an inch of our heads, detach that from the notion of chance at all, and then begin to introduce this notion again for possible deflections from that saving inch. In such a case one's prospects naturally become very disagreeable.

§ 12. If the reader will try to analyse his feelings just after he has had a narrow escape himself, or witnessed one in others, he will not unfrequently find that this fallacy is to some extent involved in them. The mere proximity to danger cannot be the cause of the anxiety, for in other cases where the danger was equally near, but from which the notion of chance is excluded, no such anxiety is felt. I do not think that any one can make any justification of his feelings, or would naturally attempt to make one, without introducing the notion of chance. We shall find it scarcely possible to explain what we feel without introducing an 'if,' or putting ourselves mentally into the same position again, and then thinking about the different issues of the event which might be expected as a general rule under those circumstances. If in such a position the probability of danger would be really great, terror would not have been inappropriate then, and therefore anxiety is a very legitimate product of imagination afterwards. But if, on the other hand, as is very often the case when all the chances are contemplated, even that amount of proximity to danger which was actually experienced was extremely improbable, then no justification can be offered for the subsequent anxiety.

§ 13. (III.) We will now notice a fallacy connected with the subjects of betting and gambling. Many or most of the popular misapprehensions on this subject imply such utter ignorance and confusion as to the foundations of the science that it would be needless to discuss them here. The following however is of a far more plausible kind, and has been a source of perplexity to persons of considerable acuteness. It appears to many to be a subtle and mysterious receipt for enabling any one to win whatever sum he pleases, provided only that he is allowed to choose on each occasion the amount of the stake to be played for.

The case, put into the simplest form, is as follows[1]. Suppose that a person *A* is playing against *B*, *B* being either another individual or a group of individuals, say gambling bank. They begin by tossing for a shilling, a *A* maintains that he is in possession of a device which w insure his winning. If he does win on the first occasion has clearly gained his point so far. If he loses, he stak next time two shillings instead of one. The result of cou is that if he wins on the second occasion he replaces former loss, and is left with one shilling profit as well. he goes on, doubling his stake after every loss, with t obvious result that on the first occasion of success he mak good all his previous losses, and is left with a shilling ov But such an occasion must come sooner or later, by t assumptions of chance on which the game is founded. Hen it follows that he can insure, sooner or later, being left final winner. Moreover he may win to any amount; firs from the obvious consideration that he might make t initial stake as large as he pleased, a hundred pounds, f instance, instead of a shilling; and secondly, because wh he has done once he may do again. He may put his shillin by, and have a second spell of play, long or short as th case may be, with the same termination to it. Accordingl by mere persistency he may accumulate any sum of mone he pleases, in apparent defiance of all that is meant by luck

---

[1] It appears to have been well known to gamblers under the name of the *Martingale*. There is a paper by Babbage (*Trans. of Royal Soc. of Edinburgh*, for 1823) which discusses certain points connected with it, but scarcely touches on the subject of the sections which follow.

§ 14. I have classed this opinion among fallacies, as th present is the most convenient opportunity of discussing i though in strictness it should rather be termed a parado since the conclusion is perfectly sound. The only fallac consists in regarding such a way of obtaining the result a mysterious. On the contrary, there is nothing more eas than to insure ultimate success under the given condition The point is worth enquiry, from the principles it involve and because the answers commonly given do not quite me the difficulty. It is sometimes urged, for instance, that n bank would or does allow the speculator to choose at will the amount of his stake, but puts a strict limit to the amount for which it will consent to play. This is quite true, but is of course no answer to the hypothetical enquiry before us, which assumes that such a state of things is allowed. Again, it may be replied, that we have no right to assume that the fortune of the player will hold out in this way, for he may be ruined before his turn of luck comes. This too is quite true, but does not explain the difficulty. We have only to suppose the men to be playing on credit to remove the objection. There is no reason whatever why any money should pass between them till the affair is finally settled. All such transactions, nearly, must be carried on to some extent on credit, unless there is to be the trouble of perpetual payments backwards and forwards; and it is therefore perfectly legitimate to suppose a state of things in which no enquiry is made as to the solvency of either of the parties until the crisis agreed upon has been reached.

§ 15. What puzzles people here is, I presume, the supposed fact that in some mysterious way certainty has been conjured out of uncertainty; that in a game where the detailed events are utterly inscrutable, and where the average, by supposition, shows no preference for either side, one party is nevertheless succeeding somehow in steadily drawing all the luck his own way. It looks to them as if it were a parallel case with that of a man who should succeed by some device in permanently securing more than half of the tosses with a penny which was nevertheless to be regarded as a perfectly fair one.

All this is quite a mistake. The real fact is that *A* does not expose his gains to chance at all; all that he so exposes is the number of times he has to wait until he gets them. Put such a case as this. I offer to give a man any sum of money he chooses to mention provided he will at once give it back again to me with one pound more. It does not need much acuteness to see that it is a matter of profound indifference to me whether he chooses to mention one pound, or ten, or a hundred. Now suppose that instead of leaving it to his choice which of these sums is to be selected each time, the two parties agree to leave it to chance. Let them, for instance, draw a number out of a bag each time, and let that be the sum which *A* gives to *B* under the prescribed conditions. The case is not altered in the least. *A* still gains his pound each time, for the introduction of the element of chance has not in any way touched this. All that it does is to make this pound the result of an uncertain subtraction sum, sometimes 10 *minus* 9, sometimes 50 *minus* 49, and so on. It is these numbers only, not their difference, which he submits to luck, and this is of no consequence whatever.

§ 16. To suggest to any individual or company that they should consent to go on playing upon such terms as these would be too barefaced a proposal for any one to dream of making it. And yet the case in question is identical in principle, and almost identical in form, with this. To offer to give a man any sum he likes to name provided he gives you back again that same sum *plus* one, and to offer him any number of terms he pleases of the series 1, 2, 4, 8, 16, &c., provided you have the last term of the set, are one and the same thing. The only difference is that in the latter case the result is attained with somewhat more of arithmetical parade. Equally identical are the processes in case we prefer to leave it to chance, instead of to choice, to decide what sum or what number of terms shall be fixed upon. This latter is what is really done in the case in question. A man who consents to go on doubling his stake every time he wins, is leaving nothing else to chance than the determination of the particular number of terms of such a geometrical series which shall be allowed to pass before he stops. The introduction of the tossing of the penny, or whatever else is the means of bringing chance to bear upon the matter, is merely an ingenious mode of mystification.

§ 17. It may be added that there is no special virtue in the particular series in question, viz. that in accordance with which the stake is doubled each time. All that is needed is that the last term of the series should more than balance all the preceding ones. Any other series which increased faster than this geometrical one, would answer the purpose as well or better. Nor is it necessary, again, that the game should be an even or 'fair' one. Chance, be it remembered, affects nothing here but the number of terms to which the series attains on each occasion, its final result being always arithmetically fixed. When a penny is tossed up it is only on every alternate occasion that the series runs to more than two terms, and so his fixed gains come in pretty regularly. But unless he was playing for a limited time only, it would not affect him if the series ran to two hundred terms; it would merely take him somewhat longer to win his stakes. A man might safely, for instance, continue to lay an *even* bet that he would get the single prize in a lottery of a thousand tickets, provided he thus doubled, or more than doubled, his stake each time, and unlimited credit was given.

§ 18. So regarded, the problem is simple enough, but there are two points in it to which attention may conveniently be directed.

In the first place, it serves very pointedly to remind us of the distinction between a series of events (in this case the tosses of the penny) which really are subjects of chance, and our conduct founded upon these events, which may or may not be so subject[1]. It is quite possible that this latter may be so contrived as to be in many respects a matter of absolute certainty,—a consideration, I presume, familiar enough to professional betting men. Why is the ordinary way of betting on the throws of a penny fair to both parties? Because a 'fair' series is 'fairly' treated. The heads and tails occur at random, but on an average equally often, and the stakes are either fixed or also arranged at random. If a man backs heads every time for the same amount, he will of course in the long run neither win nor lose. Neither will he if he varies the stake every time, provided he does not vary it in such a way as to make its amount dependent on the fact of his having won or lost the time before. But he may, if he pleases, and the other party consents, so arrange his stakes (as in the case in question) that chance, if one might so express it, does not get a fair chance. Here the human elements of choice and design have been so brought to bear upon a series of events which, regarded by themselves, exhibit nothing but the physical characteristics of chance, that the latter elements disappear, and we get a result which is arithmetically certain. Other analogous instances might be suggested, but the one before us has the merit of most ingeniously disguising the actual process.

§ 19. In the second place, this example brings before us what has had to be so often mentioned already, namely, that the series of Probability are in strictness supposed to be interminable. If therefore we allow either party to call upon us to stop, especially at a point which just happens to suit him, we may get results decidedly opposed to the integrity of the theory. In the case before us it is a necessary stipulation for *A* that he may be allowed to leave off when he wishes, that is at one of the points at which the throw is in his favour. Without this stipulation he may be left a loser to any amount.

---

[1] Attention will be further directed to this distinction in the chapter on Insurance and Gambling.

Introduce the supposition that one party may arbitrarily call for a stoppage when it suits him and refuse to permit it sooner, and almost any system of what would be otherwise fair play may be converted into a very one-sided arrangement. Indeed, in the case in question, *A* need not adopt his device of doubling the stakes every time he loses. He may play with a fixed stake, and nevertheless insure winning (or, for that matter, losing) any sum he pleases, assuming that the game is even and that he is permitted to play on credit.

§ 20. To examine the matter thoroughly would involve a mathematical calculation, but so much as follows may be readily understood by the ordinary reader. All combinations or runs of luck have their definite assignable chances; hence, if we go on long enough, every one of them will present itself sooner or later. The result of this is that the luck sways from one side to the other with greater and greater oscillations as longer scope is given. The *average* tending to remain constant, on the principles described in Ch. I., the absolute departures from the average grow greater and greater. Consider a short period, and neither party will have been much the richer or poorer during it: but consider a very long period, and each of them will find that at some time or other within it he will have been a great winner. Let *A* therefore set his heart upon winning any sum he likes to select, and all that is necessary is that he should wait till he finds himself in the desired position and *then stop*. Of course he may, if he pleases, go on further; but then he will disturb his plan, and will be under the necessity of waiting till the same position is reached again. The simple fact is that every run of luck having its determinate chance, any resultant state of wealth on either side will be reached, and recur again and again, in lapse of time. Hence he who wishes to leave off in an assigned position, has only got to bide his time till that position is attained, and then retire from the contest.

So far is there from being anything recondite in the principles here explained, that they are recognized in a rudimentary way, it may be presumed, by every player in any game that involves chance. Nothing is more common than for one of the parties to offer to his opponent, or to claim from him, his 'revenge.' This simply means that if either party is allowed to wait till the sway of luck has been more than usually in his favour, and then to refuse to go on any further, he is claiming the benefit of an accident. He has chosen (without its being clearly understood beforehand that he was entitled to choose) to leave off at a favourable moment; the supposed unfairness consisting in the fact that his opponent either has had similar favourable opportunities already and did not use them, or, at any rate, that he would have them if allowed longer time.

§ 21. (IV.) A common mistake is to assume that a very unlikely thing will not happen at all. It is a mistake which, when thus stated in words, seems too obvious to be committed, for the meaning of an unlikely thing is one that happens at rare intervals; if it were not assumed that the event would happen at rare intervals it would not be called unlikely, but impossible. This is an error which could scarcely occur except in vague popular misapprehension, and is so abundantly refuted in works on Probability, that it need only be touched upon briefly here. It follows of course, from our definition of Probability, that to speak of a very rare combination of events as one that is 'sure never to happen,' is to use language incorrectly. Such a phrase may pass current as a loose popular exaggeration, but in strictness it involves a contradiction. The truth about such rare events cannot be better described than in the following quotation from De Morgan[1]:—

"It is said that no person ever *does* arrive at such extremely improbable cases as the one just cited [drawing the same ball five times running out of a bag containing twenty balls]. That a given individual should never throw an ace twelve times running on a single die, is by far the most likely; indeed, so remote are the chances of such an event in any twelve trials (more than 2,000,000,000 to 1 against it) that it is unlikely the experience of any given country, in any given century, should furnish it. But let us stop for a moment, and ask ourselves to what this argument applies. A person who rarely touches dice will hardly believe that doublets sometimes occur three times running; one who handles them frequently knows that such is sometimes the fact. Every very practised user of those implements has seen still rarer sequences. Now suppose that a society of persons had thrown the dice so often as to secure a run of six aces observed and recorded, the preceding argument would still be used against twelve. And if another society had practised long enough to see twelve aces following each other, they might still employ the same method of doubting as to a run of twenty-four; and so on, *ad infinitum.* The power of imagining cases which contain long combinations so much exceeds that of exhibiting and arranging them, that it is easy to assign a telegraph which should make a separate signal for every grain of sand in a globe as large as the visible universe, upon the hypothesis of the most space-penetrating astronomer. The fallacy of the preceding objection lies in supposing events in number beyond our experience, composed entirely of sequences such as fall within our experience. makes the past necessarily contain the whole, as to the quality of its components; and judges by samples. Now the least cautious buyer of grain requires to examine a handful before he judges of a bushel, and a bushel before he judges of a load. But relatively to such enormous numbers of combinations as are frequently proposed, our experience does not deserve the title of a handful as compared with a bushel, or even of a single grain."

---

[1] *Essay on Probabilities*, p. 126.

§ 22. The origin of this inveterate mistake is not difficult to be accounted for. It arises, no doubt, from the exigencies of our practical life. No man can bear in mind every contingency to which he may be exposed. If therefore we are ever to do anything at all in the world, a large number of the rarer contingencies must be left entirely out of account. And the necessity of this oblivion is strengthened by the shortness of our life. Mathematically speaking, it would be said to be certain that any one who lives long enough will be bitten by a mad dog, for the event is not an impossible, but only an improbable one, and must therefore come to pass in time. But this and an indefinite number of other disagreeable contingencies have on most occasions to be entirely ignored in practice, and thence they come almost necessarily to drop equally out of our thought and expectation. And when the event is one in itself of no importance, like a rare throw of the dice, a great effort of imagination may be required, on the part of persons not accustomed to abstract mathematical calculation, to enable them to realize the throw as being even possible.

§ 23. Attempts have sometimes been made to estimate what extremity of unlikelihood ought to be considered as equivalent to this practical zero point of belief. In so far as such attempts are carried out by logicians, or by those who are unwilling to resort to mathematical valuation of chances, they must be regarded as merely a special form of the modal difficulties discussed in the twelfth chapter, and need not therefore be reconsidered here; but a word or two may be added concerning the views of some who have looked at the matter from the mathematician's point of view.

The principal of these is perhaps Buffon. He has arrived at the estimate (*Arithmétique Morale* § VIII.) that this practical zero is equivalent to a chance of $\frac{1}{10,000}$. The grounds for selecting this fraction are found in the fact that, according to the tables of mortality accessible to him, it represents the chance of a man of 56 dying in the course of the next day. But since no man under those circumstances takes the chance into the slightest consideration, it follows that it is practically estimated as having no value.

It is obvious that this result is almost entirely arbitrary, and in fact his reasons cannot be regarded as anything more than a slender justification from experience for adopting a conveniently simple fraction; a justification however which would apparently have been equally available in the case of any other fractions lying within wide limits of the one selected.

§ 24. There is one particular form of this error, which, from the importance occasionally attached to it, deserves perhaps more special examination. As stated above, there can be no doubt that, however unlikely an event may be, if we (loosely speaking) vary the circumstances sufficiently, or if, in other words, we keep on trying long enough, we shall meet with such an event at last. If we toss up a pair of dice a few times we shall get doublets; if we try longer with three we shall get triplets, and so on. However unusual the event may be, even were it sixes a thousand times running, it will come some time or other if we have only patience and vitality enough. Now apply this result to the letters of alphabet. Suppose that one at a time is drawn from a which contains them all, and is then replaced. If the letters were written down one after another as they occurred, it would commonly be expected that they would be found to make mere nonsense, and would never arrange themselves into the words of any language known to men. No more they would in general, but it is a commonly accepted result of the theory, and one which we may assume the reader to be ready to admit without further discussion, that, if the process were continued long enough, words making sense would appear; nay more, that any book we chose to mention,—Milton's *Paradise Lost* or the plays of Shakespeare, for example,—would be produced in this way at last. It would take almost as many days as we have space in this volume to represent in figures, to make tolerably certain of obtaining the former of these works by thus drawing letters out of a bag, but the desired result would be obtained at length[1]. Now many people have not unnaturally thought it derogatory to genius to suggest that its productions could have also been obtained by chance, whilst others have gone

---

[1] The process of calculation may be readily indicated. There are, say, about 350,000 letters in the work in question. Since any of the 26 letters of the alphabet may be drawn any time, the possible number of combinations would be $26^{350,000}$; a number which, as may easily be inferred from a table of logarithms, would demand for its expression nearly 500,000 figures. Only one of these combinations is favourable, if we reject variations of spelling. Hence unity divided by this number would represent the chance of getting the desired result by any one random selection of the required number of 350,000 letters.

Now if the chance of an event on each occasion is $\frac{1}{n}$, the chance of getting it once at least in n trials is $1-\left(\frac{n-1}{n}\right)^n$; for we shall do this unless we fail n times running. When (as in the case in question) n is very large, this may be shown algebraically to be equivalent to odds on to argue, If this be the case, might not the world itself in this manner have been produced by chance?

§ 25. We will begin with the comparatively simple, determinate, and intelligible problem of the possible production of the works of a great human genius by chance. With regard to this possibility, it may be a consolation to some timid minds to be reminded that the power of producing the works of a Shakespeare, *in time*, is not confined to consummate genius and to mere chance. There is a third alternative, viz. that of purely mechanical procedure. Any one, down almost to an idiot, might do it, if he took sufficient time about the task. For suppose that the required number of letters were procured and arranged, not by chance, but designedly, and according to rules suggested by the theory of permutations: the letters of the alphabet and the number of them to be employed being finite, every order in which they could occur would come in its due turn, and therefore every thing which can be expressed in language would be arrived at some time or other; the works of Shakespeare of course amongst other things. It might take as long to do it in one of these irregular ways as in the other, but with unlimited time either plan would be feasible.

There is really nothing that need startle or shock any one in such a result. Its possibility arises from the following cause. The number of letters, and therefore of words, at our disposal is limited; whatever therefore we may desire to express in language necessarily becomes subject to corresponding limitation. The possible variations of thought are literally infinite, so are those of spoken language (by intonation of

of about 2 to 1. That is, when we have drawn the requisite quantity of letters a number of times equal to the inconceivably great number above represented, it is still only 2 to 1 that we shall have secured what we want.

the voice, &c.); but when we come to words there is a
ation, the nature of which is distinctly conceivable
mind, though the restriction is one that in practi
never be appreciable, owing to the fact that the num
combinations which may be produced is so enormous
surpass all power of the imagination to realize[1]. The
therefore is plain, and it is one that will apply to man
cases as well, that to put a finite limit upon the nun
ways in which a thing can be done, is to determine th
one who is able and willing to try long enough shall s
in doing it. If a great genius condescends to perf
under these circumstances, he must submit to the pos
of having his claims rivalled or disputed by the chan
and idiot. If Shakespeare were limited to the use o
or nine assigned words, the time within which the
agents might claim equality with him would not b
great. As it is, having had the range of the Engli
guage at his disposal, his reputation is not in danger
assailed by any such methods.

As an additional security it may be remarked, in r
to both of these latter agents, that, even when in
time they had stumbled upon a brilliant concept
would probably not be aware of the fact; so that
they would require a Shakespeare at their elb
them at which of their performances they had bett

§ 26. The case of the possible production of
by chance leads us into an altogether different re
cussion. We are not here dealing with figures
and use of which are within the fair powers of
standing, however the imagination may break

---

[1] The longest life which could reasonably be attributed to any language would of course dwindle into utter insignificance
such periods of ti
here arithmetically

tempting to realize the smallest fraction of their full significance. The understanding itself is wandering out of its proper province, for the conditions of the problem cannot be assigned. When we draw letters out of a bag we know very well what we are doing, but what is really meant by producing a world by chance? By analogy of the former case, we may assume that some kind of agent is presupposed;—perhaps therefore the following supposition is less absurd than any other. Imagine some being, not a Creator but a sort of Demiurgus, who has had a quantity of materials put into his hands, and he assigns them their collocations and their laws of action, blindly and at haphazard: what are the odds that such a world as we actually experience should have been brought about in this way?

If it were worth while seriously to set about answering such a question, and if some one would furnish us with the number of the letters of such an alphabet, and the length of the work to be written with them, we could proceed to indicate the result. But so much as this may surely be affirmed about it;—that, far from merely finding the length of this small volume insufficient for containing the figures in which the adverse odds would be given, all the paper which the world has hitherto produced would be used up before we had got far on our way in writing them down.

The question would hardly have been worth noticing at all, except as a good opportunity of reminding the reader of the somewhat artificial nature of the distinction between necessary and contingent truth. The moment we admit a possibility, people seem to think that this marks a distinction in kind from that which they regard as impossible. But the fact is that no proposition, even of the kind that we use as typical of certainty, approaches in cogency to the denial that this world was (under the above suppositions) produced by chance. Take, for example, the simplest proposition in Euclid. The beginner notoriously is apt to make mistakes there; we cannot therefore be certain that the advanced mathematician may not do so too, nor therefore that any number of them may not equally fail. Whatever figure be put upon these contingencies we may safely say that the result stands in point of cogency as far below the non-production of a chance world, as it stands above what we commonly regard as utterly doubtful events.

§ 27. There is another form of this practical inability to distinguish between one high number and another in the estimation of chances, which deserves passing notice from its importance in arguments about heredity. People will often urge an objection to the doctrine that qualities, mental and bodily, are transmitted from the parents to the offspring, on the ground that there are a multitude of instances to the contrary, in fact a great majority of such instances. To raise this objection implies an utter want of appreciation of the very great odds which possibly may exist, and which the argument in support of heredity implies *do* exist against any given person being distinguished for intellectual or other eminence. This is doubtless partly a matter of definition, depending upon the degree of rarity which we consider to be implied by eminence; but taking any reasonable sense of the term, we shall readily see that a very great proportion of failures may still leave an enormous preponderance of evidence in favour of the heredity doctrine. Take, for instance, that degree of eminence which is implied by being one of four thousand. This is a considerable distinction, though, since there are about two thousand such persons to be found amongst the total adult male population of Great Britain, it is far from implying any conspicuous genius. Now suppose that in examining the cases of a large number of the chilren of such persons, we had found that 199 out of 200 of them failed to reach the same distinction. Many persons would conclude that this was pretty conclusive evidence against any hereditary transmission. To be able to adduce only one favourable, as against 199 hostile instances, would to them represent the entire break-down of any such theory. The error, of course, is obvious enough, and one which, with the figures thus before him, hardly any one could fail to avoid. But if one may judge from common conversation and other such sources of information, it is found in practice exceedingly difficult adequately to retain the conviction that even though only one in 200 instances were favourable, this would represent odds of about 20 to 1 in favour of the theory. If hereditary transmission did not prevail, only one in 4000 sons would thus rival their fathers; but we find actually, let us say, (we are of course taking imaginary proportions here), that one in 200 does. Hence, if the statistics are large enough to be satisfactory, there has been some influence at work which has improved the chances of mere coincidence in the ratio of 20 to 1. We are in fact so little able fully to realise the meaning of very large numbers, that unless we repeatedly check ourselves by arithmetical considerations we are too apt to treat and estimate all beyond certain limits as equally vast and vague. When we come to deal with millions instead of with thousands this tendency becomes of course much stronger.

§ 28. (V.) In discussing the nature of the connexion between Probability and Induction, we examined the claims of a rule commonly given for inferring the probability that an event which had been repeatedly observed would recur again. I endeavoured to show that all attempts to obtain and prove such a rule were necessarily futile; if these reasons were conclusive the employment of such a rule must of course be regarded as fallacious. A few examples may c veniently be added here, tending to show how instea there being merely a single rule of succession we might b ter divide the possible forms of the rule into three classes.

(1) In some cases when a thing has been observed happen several times it becomes in consequence *more* likely that the thing should happen again. This agrees with *th*e ordinary form of the rule, and is probably the case of most frequent occurrence. The necessary vagueness of expression when we talk of the 'happening of a thing' makes it quite impossible to tolerate the rule in this general form, but if we specialize it a little we shall find it assume a more familiar shape. If, for example, we have observed two or more properties to be frequently associated together in a succession of individuals, we shall conclude with some force that they will be found to be so connected in future. The strength of our conviction however will depend not merely on the number of observed coincidences, but on far more complicated considerations; for a discussion of which the reader must be referred to regular treatises on Inductive evidence. Or again, if we have observed one of two events succeed the other several times, the occurrence of the former will excite in most cases some degree of expectation of the latter. As before, however, the degree of our expectation is not to be assigned by any simple formula; it will depend in part upon the supposed intimacy with which the events are connected. To attempt to lay down definite rules upon the subject would lead to a discussion upon laws of causation, and the circumstances under which their existence may be inferred, and therefore any further consideration of the matter must be abandoned here.

§ 29. (2) Or, secondly, the past recurrence may in itself give no valid grounds for inference about the future;

his is the case which most properly belongs to Probability[1]. hat it does so belong will be easily seen if we bear in mind the fundamental conception of the science. We are there introduced to a series,—for purposes of inference an indefinitely extended series,—of terms, about the details of which, information, except on certain points, is not given; our knowledge being confined to the statistical fact, that, say, one in ten of them has some attribute which we will call *X*. Suppose now that five of these terms in succession have been *X*, what hint does this give about the sixth being also an *X*? Clearly none at all; this past fact tells us nothing; the formula for our inference is still precisely what it was before, that one in ten being *X* it is one to nine that the next term is *X*. And however many terms in succession had been of one kind, precisely the same formula would still be given.

§ 30. The way in which events will justify the answer given by this formula is often misunderstood. For the benefit therefore of those unacquainted with some of the conceptions familiar to mathematicians, a few words of explanation may be added. Suppose then that we have had *X* twelve times in succession. This is clearly an anomalous state of things. To suppose anything like this continuing repeatedly to occur would be obviously in opposition to the statistics, which assert that in the long run only one in ten is *X*. But how is this anomaly got over? In other words, how do we obviate the conclusion that *X*'s will occur more frequently than once in ten times, after such a long succession of them as we have now had? Many people seem to believe that there must be a diminution of X's afterwards to counterbalance their past preponderance. This however would be quite a mistake; the proportion in which they occur in future must remain the same throughout; it cannot be altered if we are to adhere to our statistical formula. The fact is that the rectification of the exceptional disturbance in the proportion will be brought about simply by the continual influx of fresh terms in the series. These will in the long run neutralize the disturbance, not by any special adaptation, as it were, for the purpose, but by the mere weight of their overwhelming numbers. At every stage therefore, in the succession, whatever might have been the number and nature of the preceding terms, it will still be true to say that one in ten of the terms will be an X.

---

[1] We are here assuming of course that the ultimate limit to which our average tends is known, either from knowledge of the causes or from previous extensive experience. We are assuming that *e.g.* the die is known to be a fair one; if this is not known, but a possible bias has to be inferred from its observed performances, the case falls under the last head.

§ 31. If we had to do only with a finite number of terms, however large that number might be, such a disturbance as we have spoken of would, it is true, need a special alteration in the subsequent proportions to neutralize its effects. But when we have to do with an infinite number of terms, this is not the case; the 'limit' of the series, which is what we then have to deal with (Ch. v. § 36), is entirely unaffected by these temporary disturbances. In the continued progress of the series we shall find, as a matter of fact, more and more of such disturbances, and these of a more and more exceptional character. But whatever the point we may occupy at any time, if we look forward or backward into the indefinite extension of the series, we shall still see that the ultimate limit to the proportion in which its terms are arranged remains the same; and it is with this limit, as above mentioned, that we are concerned in the strict rules of Probability. Suppose, for illustration, that there is one part in a thousand of salt in sea-water. Put some fresh water in a cistern; then however much there might have been of the fresh to begin with, if only salt water enough is turned on, the proportion that will be ultimately approximated to will be the same as that in the sea itself, viz. one part in a thousand of salt. To effect this it will not be necessary to suppose the subsequent sea-water to be more salt; the proportion will be restored, or, to speak strictly, will tend to be restored simply by continuing for ever to add water of the original degree of saltness.

The most familiar example, perhaps, of this kind is that of tossing up a penny. Suppose we have had four heads in succession; people have tolerably realized by now that 'head the fifth time' is still an even chance, as 'head' was each time before, and will be ever after. The preceding paragraph explains how it is that these occasional disturbances in the average become neutralized in the long run.

§ 32. (3) There are other cases which, though rare, are by no means unknown, in which such an inference as that obtained from the Rule of Succession would be the direct reverse of the truth. The oftener a thing happens, it may be, the more unlikely it is to happen again. This is the case whenever we are drawing things from a limited source (as balls from a bag without replacing them), or whenever the act of repetition itself tends to prevent the succession (as in giving false alarms).

I am quite ready to admit that we believe the results described in the last two classes on the strength of some such general Inductive rule, or rather principle, as that involved in the first. But it would be a great error to confound this with an admission of the validity of the rule in each special instance. We are speaking about the application of the rule to individual cases, or classes of cases; this is quite a distinct thing, as was pointed out in a previous chapter, from giving the grounds on which we rest the rule itself. If a man were to lay it down as a universal rule, that the te mony of all persons was to be believed, and we adduced instance of a man having lied, it would not be conside that he saved his rule by shewing that we believed that was a lie on the word of other persons. But it is perfec consistent to give as a merely general, but not univers rule, that the testimony of men is credible; then to separa off a second class of men whose word is not to be trust and finally, if any one wants to know our ground for t second rule, to rest it upon the first. If we were speaki of *necessary* laws, such a conflict as this would be hopeless as the old 'Cretan' puzzle in logic; but in i stances of Inductive and Analogical extension it is perfect harmless.

§ 33. A familiar example, about which many peop must have disputed at one time or another of their liv will serve to bring out the three different possible conclusio mentioned above. We have observed it rain on ten successi days. *A* and *B* conclude respectively for and against rain the eleventh day; *C* maintains that the past rain affords data whatever for an opinion. Which is right? We rea cannot determine *à priori*. An appeal must be made direct observation, or means must be found for deciding independent grounds to which class we are to refer t instance. If, for example, it were known that every count produces its own rain, we should choose the third rule, for would be a case of drawing from a limited supply. If aga we had reasons to believe that the rain for our country mig be produced anywhere on the globe, we should probably co clude that the past rainfall threw no light whatever on t prospect of a continuance of wet weather, and therefore ta the second. Or if, finally, we knew that rain came in lo spells or seasons, as in the tropics, to have had ten wet da in succession would make us believe that we had entered on one of these seasons, and that therefore the next day would probably resemble the preceding ten.

Since then all these forms of such an Inductive rule are possible, and we have often no *à priori* grounds for preferring one to another, it would seem to be unreasonable to attempt to establish any universal formula of anticipation. All that we can do is to ascertain what are the circumstances under which one or other of these rules is, as a matter of fact, found to be applicable, and to make use of it under those circumstances.

§ 34. (VI.) In the cases discussed in (IV.) the almost infinitely small chances with which we were concerned were rightly neglected from all practical consideration, however proper it might be, on speculative grounds, to keep our minds open to their actual existence. But it has often occurred to me that there is a common error in neglecting to take them into account when they may, though individually small, make up for their minuteness by their number. As the mathematician would express it, they may occasionally be capable of being *integrated* into a finite or even considerable magnitude.

For instance, we may be confronted with a difficulty out of which there appears to be only one appreciably possible mode of escape. The attempt is made to force us into accepting this, however great the odds apparently are against it, on the ground that improbable as it may seem, it is at any rate vastly more probable than any of the others. I can quite admit that, on practical grounds, we may often find it reasonable to adopt this course; for we can only *act* on one supposition, and we naturally and rightly choose, out of a quantity of improbabilities, the least improbable. But when we are not forced to act, no such decisive preference is demanded of us. It is then perfectly reasonable to refuse assent to the proposed explanation; even to say distinctly that we do not believe it, and at the same time to decline, of course, to accept any other explanation. We remain, in fact, in a state of suspense of judgment, a state perfectly right and reasonable so long as no action demanding a specific choice is forced upon us. One alternative may be decidedly probable as compared with any other individually, but decidedly improbable as compared with all others collectively. This in itself is intelligible enough; what people often fail to see is that there is no necessary contradiction between saying and feeling this, and yet being prepared vigorously to act, when action is forced upon us, as though this alternative were really the true one.

§ 35. To take a specific instance, this way of regarding the matter has often occurred to me in disputes upon 'Spiritualist' manifestations. Assent is urged upon us because, it is said, no other possible solution can be suggested. It may be quite true that apparently overwhelming difficulties may lie as against each separate alternative solution; but is it always sufficiently realized how extremely numerous such solutions may be? No matter for their being almost incredible, or even absurd, they ought all to be massed together and thrown into the scale against the proffered solution, when the only question asked is, Are we to accept this solution? There is no unfairness in such a course. We are perfectly ready to adopt the same plan against any other individual alternative, whenever any person or party takes to announcing that as *the* solution of the difficulty. We are looking at the matter from a purely logical point of view, and are quite willing, so far, to place every solution, spiritualist or otherwise, upon the same footing. The partisans of every alternative are in somewhat the same position as the members of a deliberative assembly, in which no one will support the motion of any other member. Every one can aid effectively in rejecting every other motion, but no one can succeed in passing his own. Pressure of urgent necessity may possibly force them out of this state of practical inaction, by, so to say, breaking through the opposition at some point of least resistance; but unless aided by some such pressure they are left in a state of hopeless dead-lock.

§ 36. Assuming that the spiritualistic solution admits of, and is to receive, scientific treatment, this, it seems to me, is the conclusion to which one might sometimes be led in the face of the evidence offered. We might have to say to every individual explanation, It is incredible, I cannot accept it; and unless circumstances should (which it is hardly possible that they should) force us to a hasty decision,—a decision, remember, which need indicate no preference of the judgment beyond what is just sufficient to turn the scale in its favour as against any other single alternative, we leave the matter thus in abeyance. It will very likely be urged that one of the explanations (assuming that all the possible ones had been included) must be true; this we readily admit. It will probably also be urged that (on the often-quoted principle of Butler) we ought forthwith to accept the one which, as compared with the others, is the most plausible, whatever its absolute worth may be. This seems distinctly an error. To say that such and such an explanation is the one we should accept, *if* circumstances compelled us to anticipate our decision, is quite compatible with its present absolute rejection. The only rational position surely is that of admitting that the truth is somewhere amongst the various alternatives, but confessing plainly that we have no such preference for one over another as to permit our saying anything else than that we disbelieve every one of them.

§ 37. (VII.) The very common fallacy of 'judging by the event,' as it is generally termed, deserves passing notice here, as it clearly belongs to Probability rather than to Logic; though its nature is so obvious to those who have grasped the general principles of our science, that a very few words of remark will suffice. In one sense every proposition must consent to be judged by the event, since this is merely, in other words, submitting it to the test of experience. But there is the widest difference between the test appropriate to a universal proposition and that appropriate to a merely proportional or statistical one. The former is subverted by a single exception; the latter not merely admits exceptions, but implies them. Nothing, however, is more common than to blame advice (in others) because it has happened to turn out unfortunately, or to claim credit for it (in oneself) because it has happened to succeed. Of course if the conclusion was avowedly one of a probable kind we must be prepared with complacency to accept a hostile event, or even a succession of them; it is not until the succession shows a disposition to continue over long that suspicion and doubt should arise, and then only by a comparison of the degree of the assigned probability, and the magnitude of the departure from it which experience exhibits. For any single failure the reply must be, 'the advice was sound' (supposing, that is, that it was to be justified in the long run), 'and I offer it again under the same circumstances.'

CHAPTER XV.

INSURANCE AND GAMBLING.

§ 1. If the reader will recall to mind the fundamental postulate of the Science of Probability, established and explained in the first few chapters, and so abundantly illustrated since, he will readily recognize that the two opposite characteristics of individual irregularity and average regularity will naturally be differently estimated by different minds. To some persons the elements of uncertainty may be so painful, either in themselves or in their consequences, that they are anxious to adopt some means of diminishing them. To others the ultimate regularity of life, at any rate within certain departments, its monotony as they consider it, may be so wearisome that *they* equally wish to effect some alteration and improvement in its characteristics. We shall discuss briefly these mental tendencies, and the most simple and obvious modes of satisfying them.

To some persons, as we have said, the world is all too full of change and irregularity and consequent uncertainty. Civilization has done much to diminish these characteristics in certain directions, but it has unquestionably aggravated them in other directions, and it might not be very easy to say with certainty in which of these respects its operation has been on the whole most effective. The diminution of irregularity is exemplified, amongst other things, in the case of the staple products which supply our necessary food and clothing. With respect to them, famine and scarcity are by comparison almost unknown now, at any rate in tolerably civilized communities. As a consequence of this, and of the vast improvements in the means of transporting goods and conveying intelligence, the fluctuations in the price of such articles are much less than they once were. In other directions, however, the reverse has been the case. Fashion, for instance, now induces so many people in every large community simultaneously to desire the same thing, that great fluctuations in value may ensue. Moreover a whole group of causes (to enter upon any discussion of which would be to trench upon the ground of Political Economy) combine to produce great and frequent variations in matters concerning credit and the currency, which formerly had no existence. Bankruptcy, for instance, is from the nature of the case, almost wholly a creation of modern times. Machinery also, in railways, manufactures, and so on, has introduced uncertainties in respect of life and limb, such as the less enterprising experience of former days could scarcely furnish. We will not attempt to strike any balance between these opposite results of modern civilization, beyond remarking that in matters of prime importance the actual uncertainties have been probably on the whole diminished, whereas in those which affect the pocket rather than the life, they have been rather increased. It might also be argued with some plausibility that in cases where the actual uncertainties have not become greater, they have for all practical purposes done so, by their consequences frequently becoming more serious, or by our estimate of these consequences becoming higher.

§ 2. However the above question, as to the ultimate balance of gain or loss, should be decided, there can be no doubt that many persons find the present amount of uncertainty in some of the affairs of life greater than suits their taste. How are they to diminish it? Something of course may be done, as regards the individual cases, by prudence and foresight. Our houses may be built with a view not to take fire so readily, or precautions may be taken that there shall be fire engines at hand. In the warding off of death from disease and accident, something may be done by every one who chooses to live prudently. Precautions of the above kind, however, do not introduce any questions of Probability. These latter considerations only come in when we begin to invoke the regularity of the average to save us from the irregularities of the details. We cannot, it is true, remove the uncertainty in itself (for we are talking now of the irremovable part of it, after all has been done that can be done in the way of equalization), but we can so act that the consequences of that uncertainty shall be less to us, or to those in whom we are interested. Take the case of Life Insurance. A professional man who has nothing but the income he earns to depend upon, knows that the whole of that income may vanish in a moment by his death. This is a state of things which he cannot prevent; and if he were the only one in such a position, or were unable or unwilling to combine with his fellow-men, there would be nothing more to be done in the matter except to live within his income as much as possible, and so leave a margin of savings.

§ 3. There is however an easy mode of escape for him. All that he has to do is to agree with a number of others, who are in the same position as himself, to make up, so to say, a common purse. They may resolve that those of their number who live to work beyond the average length of life shall contribute to support the families of those who die earlier. If a few only concurred in such a resolution they would not gain very much, for they would still be removed by but a slight step from that uncertainty which they are seeking to escape. What is essential is that a considerable number should thus combine so as to get the benefit of the comparative regularity which the average, as is well known almost always tends to exhibit.

§ 4. The above simple considerations really contain the essence of all insurance. Such points as the fact that the agreement for indemnity extends only to a certain definite sum of money; and that instead of calling for an occasional general contribution at the time of the death of each member they substitute a fixed annual premium, out of the proceeds of which the payment is to be made, are merely accidents of convenience and arrangement. Insurance is simply equivalent to a contract amongst those who dread the consequences of the uncertainty of their life or employment, that they will employ the aggregate regularity to neutralize the individual irregularity. They must know that for every one who gains by such a contract another will lose as much; or if one gains a great deal many must have lost a little. They know also that hardly any of their number can expect to find the arrangement a 'fair' one, in the sense that they may hope just to get back again what they have paid in premiums, after deducting the necessary expenses of management; but they deliberately prefer this state of things. They consist of a body of persons who think it decidedly better to leave behind them a comparatively fixed fortune, rather than one which is extremely uncertain in amount; although they are perfectly aware that owing to the unavoidable expenses of managing the affairs of such a society, the comparatively fixed sum so to be left, will be a trifle less than the average fortunes which would have been left had no such system of insurance been adopted.

As this is not a regular treatise upon Insurance no more need be said upon the exact nature of such societies, beyond pointing out that they are of various different kinds. Sometimes they really are what we have compared them with, viz. mutual agreements amongst a group of persons to make up each others losses to a certain extent. Into this category fall the Mutual Insurance Societies, Benefit Societies, Trades Unions (in respect of some of their functions), together with innumerable other societies which go by various names. Sometimes they are companies worked by proprietors or shareholders for a profit, like any other industrial enterprise. This is the case, I believe, with the majority of the ordinary Life Insurance Societies. To this class may be referred the 'Underwriting' business of ship insurance, for though each individual insurer (we are referring of course to those who *accept* the insurance, not to the ship-owners) undertakes a sole and not a corporate responsibility, yet they take the precaution of subdividing each risk amongst themselves. Hence they obtain on a small and private scale the same security which would otherwise only be possible to a large company. Sometimes, again, it is the State which undertakes the management, as in the case of our Post Office Insurance business.

§ 5. It is clear that there is no necessary limit to the range of application of this principle. It is quite conceivable that a whole nation might be so enamoured of security that they should devise a grand insurance society to cover almost every concern in life[1]. They could not indeed abolish uncertainty, for the conditions of life are very far from permitting this, but they could without much difficulty get rid of the worst of the *consequences* of it. They might determine to insure not merely their lives, houses, ships, and other things in respect of which sudden and total loss is possible, but also to insure their business; in the sense of avoiding not only bankruptcy, but even casual bad years, on the same principle of commutation. Unfamiliar as such an aim may appear when introduced in this language, it is nevertheless one which under a name of grim import to the conservative classes has had a good deal of attention directed to it. It is really scarcely anything else than Communism, which might indeed be defined as a universal and compulsory[1] insurance society which is to take account of all departments of business, and, in some at least of its forms, to invade the province of social and domestic life as well.

---

[1] The question of the advisability of inoculation against the small-pox, which gave rise to much discussion amongst the writers on Probability during the last century, is a case in point of the same principles applied to a very different kind of instance. The loss against which the insurance was directed was death by small-pox, the premium paid was the illness and other inconvenience, and the very small risk of death, from the inoculation. The disputes which thence arose amongst writers on the

Although nothing so comprehensive as this is likely to be practically carried out on any very large scale, it deserves notice that the principle itself is steadily spreading in every direction in matters of detail. It is, for instance, the great complaint against Trades Unions that they too often seek to

---

subject involved the same difficulties as to the balance between certain moderate loss and contingent great loss. In the seventeenth century it seems to have been an occasional practice, before a journey into the Mediterranean, to insure against capture by Moorish pirates, with a view to secure having the ransom paid. (See, for an account of some extraordinary developments of the insurance principle, Walford's *Insurance Guide and Handbook*. It is not written in a very scientific spirit, but it contains much information on all matters connected wit insurance.)

[1] All that is meant by the abo comparison is that the ideal aim at by Communism is similar to th of Insurance. If we look at processes by which it would carried out, and the means for forcing it, the matter would of co assume a very different as Similarly with the action of Tr Unionism referred to in the paragraph.

secure these results in respect of the equalization of the workmen's wages, thus insuring to some degree against incompetence, as they rightly and wisely do against illness and loss of work. There is the Tradesmen's Mutual Protection Society, which insures against the occasional loss entailed by the necessity of having to conduct prosecutions at law. There are societies in many towns for the prosecution of petty thefts, with the object of escaping the same uncertain and perhaps serious loss. Amongst instances of insurance *for* the people rather than *by* them, there is of course the giant example of the English Poor Law, in which the resemblance to an initial Communistic system becomes very marked. The poor are insured against loss of work arising not only from illness and old age, but from any cause except wilful idleness. They do not, it is true, pay the whole premium, but since they bear some portion of the burden of national taxation they must certainly be considered as paying a part of the premium. In some branches also of the public and private services the system is adopted of deducting a percentage from the wage or salary, for the purpose of a semi-compulsory insurance against death, illness or superannuation.

§ 6. Closely connected with Insurance, as an application of Probability, though of course by contrast, stands Gambling. Though we cannot, in strictness, term either of these practices the converse of the other, it seems nevertheless correct to say that they spring from opposite mental tendencies. Some persons, as has been said, find life too monotonous for their taste, or rather the region of what can be predicted with certainty is too large and predominant in their estimation. They can easily adopt two courses for securing the changes they desire. They may, for one thing, aggravate and intensify the results of events which are comparatively incapable of prevision, these events not being in themselves of sufficient importance to excite any strong emotions. The most obvious way of doing this is by betting upon them. Or again, what is a still more frequent course, they may invent games or other pursuits, the individual contingencies of which are entirely removed from all possible human prevision, and then make heavy money consequences depend upon these contingencies. This is gambling proper, carried on mostly by means of cards and dice. No one who knows anything of the history of manners and customs will need to be informed what an enormous aggregate of human life and interest such pursuits as these have altogether represented.

The gambling spirit, as we have said, seeks for the excitement of uncertainty and variety. When therefore people make a long continued practice of playing, especially if the stakes for which they play are moderate in comparison with their fortune, this uncertainty from the nature of the case begins to diminish. The thoroughly practised gambler, if he possesses more than usual skill (in games where skill counts for something), must be regarded simply as a man following a profession, though a profession for the most part of a risky and exciting kind. If, on the other hand, his skill is below the average, or the game is one in which skill does not tell and the odds are slightly in favour of his antagonist, as in the gaming tables, the only light in which he can be regarded is that of a man who is following a favourite amusement; if this amusement involves a constant annual outlay on his part, that is nothing more than what has to be said of most other amusements.

§ 7. We cannot, of course, give such a rational explanation as the above in every case. There are plenty of novices, and plenty of fanatics, who go on steadily losing in the full conviction that they will eventually come out winners. But it is hard to believe that such ignorance, or such intellectual twist, can really be so all-prevalent as would be requisite to constitute them the rule rather than the exception. There must surely be some very general impulse which is gratified by such resources, and it is not easy to see what else this can be than a love of that variety and consequent excitement which can only be found in perfection where exact prevision is absolutely impossible to any finite intelligence. Any rational motives of the kind which could be tested by experience, must, one would think, give way at last under the pressure of continued adverse experience.

It is true that it is not always found easy to maintain that steady uniform persistence which thus converts the practice of gambling into a profession or an amusement, as the case may be. It is remarked that the gambler, if he continues to play for a long time, is under an almost irresistible impulse to increase his stakes, and so re-introduce the element of uncertainty. It is in fact this tendency to be thus led on, which makes the principal danger and mischief of the practice. Risk and uncertainty are still such normal characteristics of even civilized life, that the mere extension of such properties into new fields does not in itself offer any very alarming prospect. It is only to be deprecated in so far as there is a danger, which experience shows to be no trifling one, that the fascination found in the pursuit should lead men into following it up into excessive lengths.

§ 8. The above seems to me the only rational and sound principle of division;—namely, that on which the different practices which, under various names, are known as gambling or insurance, are arranged in accordance with the spirit of which they are the outcome, and therefore of the results which they are designed to secure. If we were to attempt to judge and arrange them according to the which they bear, we should find ourselves led to no systematic division whatever; the fact being that sin all alike involve, as their essential characteristic, pa and receipts, one or both of which are uncertain in the or amount, they may be made to change names almos criminately.

For instance, a lottery and an ordinary insurance against accident, if we merely look to the process formed in them, are to all intents and purposes id In each alike there is a small payment which is certa a great receipt which is uncertain. A great many pay the small premium, and one or two only of their obtain a prize, the rest getting no return whatever f outlay. In each case alike, also, the aggregate recei losses are intended to balance one another, after allov the profits of those who carry on the undertaking. course when we take into account the occasions upo the insurers get their prizes, we see that there is all ference in the world between receiving them at ha as in a lottery, and receiving them as a partial set- broken limb or injured constitution, as in the in society.

Again, the language of betting may be easily n cover almost every kind of insurance. Indeed De has described life insurance as a bet which the in makes with the company, that he will not live be certain age. If he dies young, he is pecuniarily a g he dies late he is a loser[1]. Here, too, though the ex is technically quite correct, since any such deliberate [illegible] of money, upon an unproductive venture, must fall under [illegible] definition of a bet; there is the broadest distinction [illegible] betting with no other view whatever than that of [illegible] money, and betting with the view of diminishing [illegible] loss as much as possible. In fact if the language of [illegible] life is to be introduced into the matter at all [illegible] I presume, to speak of the insurer as betting against [illegible] death.

---

[1] "A fire insurance is a simple bet between the office and the party, and a life insurance is a collection of wagers. There is something of the principle of a wager in every transaction in which the a future event are to bri loss." *Penny Cyclopædia*, head of *Wager*.

§ 9. Again, in Tontines we have a [illegible] often called Insurance, and in certain [illegible] but which is to all intents and purposes [illegible] lutely a gambling transaction. [illegible] abandoned, I believe, for some time [illegible] popular, especially in France. [illegible] whatever society manages the business [illegible] thing until the last member of the [illegible] number of the survivors diminishes [illegible] annuities still continues to be paid [illegible] any are left alive, so that each receives a [illegible] sum. Hence those who die early [illegible] most, as on the ordinary plan, [illegible] death of each member the annuity [illegible] as he and his relations are concerned. [illegible] therefore is to all intents and purposes a [illegible] of betting, to see which can live [illegible] being the common stake-holder, and [illegible] sion for its superintendence, this [illegible] its sole motive for encouraging such a [illegible] recorded of one of the French Tontines [illegible] was left, as the last survivor, to receive an [illegible]

---

[1] *Encyclopédie Méthodique*, under the head of [illegible]

livres during the rest of the life which she could manage t drag on after that age;—she having originally subscribed single sum of 300 livres only. It is obvious that such a system as this, though it may sometimes go by the name of insurance, is utterly opposed to the spirit of true insurance, since it tends to aggravate existing inequalities of fortune instead of to mitigate them. The insurer here bets that he will die old; in ordinary insurance he bets that he will die young.

Again, to take one final instance, common opinion often regards the bank or company which keeps a *rouge et noir* table, and the individuals who risk their money at it, as being both alike engaged in gambling. So they may be, technically, but for all practical purposes such a bank is as sure and safe a business as that of any ordinary insurance society, and probably far steadier in its receipts than the majority of ordinary trades in a manufacturing or commercial city. The bank goes in for many and small transactions, in proportion to its capital; their customers, very often, in proportion to their incomes, for very heavy transactions.

§ 10. We have so far regarded Insurance and Gambling as being each the product of a natural impulse, and as having each, if we look merely to experience, a great mass of human judgment in its favour. The popular moral judgment, however, which applauds the one and condemns the other rests in great part upon an assumption, which has doubtless much truth in it, but which is often interpreted with an absoluteness which leads to error in each direction;—the duty of insurance being too peremptorily urged upon every one, and the practice of gambling too universally regarded as involving a sacrifice of real self-interest, as being in fact little better than a persistent blunder. The assumption in question seems to be extracted from the acknowledged advantages of insurance, and then invoked to condemn gambling. But in so doing the fact does not seem to be sufficiently recognized that the latter practice, if we merely look to the extent and antiquity of the tacit vote of mankind in its favour, might surely claim to carry the day.

It is of course obvious that in all cases with which we are concerned, the aggregate wealth is unaltered; money being merely transferred from one person to another. The loss of one is precisely equivalent to the gain of another. At least this is the approximation to the truth with which we find it convenient to start[1]. Now if the happiness which is yielded by wealth were always in direct proportion to its amount, it is not easy to see why insurance should be advocated or gambling condemned. In the case of the latter this is obvious enough. I have lost £50, say, but others (one or more as the case may be) have gained it, and the increase of their happiness would exactly balance the diminution of mine. In the case of Insurance there is a slight complication, arising from the fact that the falling in of the policy does not happen at random (otherwise, as already pointed out, it would be simply a lottery), but is made contingent upon some kind of loss, which it is intended as far as possible to balance. I insure myself on a railway journey, break my leg in an accident, and, having paid threepence for my ticket, receive say £200 compensation from the insurance company. The same remarks, however, apply here; the happiness I acquire by this £200 would only just balance the aggregate loss of the 16,000 who have paid their threepences and received no return for them, were happiness always directly proportional to wealth.

---

[1] Of course, if we introduce considerations of Political Economy, corrections will have to be made. For one thing, every Insurance Office is, as De Morgan repeatedly insists, a *Savings Bank* as well as an Insurance Office. The Office invests the premiums, and can therefore afford to pay a larger sum than would otherwise be the case. Again, in the case of gambling, a large loss of capital by any one will almost necessarily involve an actual destruction of wealth; to say nothing of the fact that, practically, gambling often causes a constant transfer of wealth from productive to unproductive purposes.

§ 11. It seems clear therefore that the general prevalence of Insurance adds confirmation to the common assumption, that equality of wealth is on the whole a desirable thing; that is, that loss of money is intrinsically more painful than gain of it is pleasurable, and that happiness does not increase in the same ratio as the material sources of it are increased. There is, it must be admitted, a certain amount of direct psychological warrant for this assumption; and the practice of Insurance is a decided confirmation of it, not so much from its wide prevalence (in which respect, as already remarked, it must yield to gambling) as from the deliberation with which it is both performed at the time and also justified afterwards, and the generally thoughtful character of its advocates.

But this general admission is all that we can grant. We must unhesitatingly reject the attempts of various mathematical writers to assign a law according to which the pleasure is supposed to vary, and thence to give actual demonstration of the fact of gambling entailing a loss, and even to measure the amount of the loss. Whilst we are on this part of the subject it may be well to give a brief notice of some of these attempts. They all involve the same distinction between what are called, under one name or another, the moral fortune and the physical fortune respectively of any person; the latter being the actual sum itself, the former its presumed worth or value to its owner. When, therefore, a given sum is gained, the 'moral' gain may be very different from what it would be to another person; and the same sum, gained and lost, respectively, may have very different values.

§ 12. With regard to the exact relation of this moral fortune to the physical, various more or less arbitrary assumptions have been made[1]. One writer (Buffon) makes the moral value of any given sum vary inversely with the total wealth of the owner; another (D. Bernoulli), from a somewhat different assumption, makes the moral worth of a fortune vary with the logarithm of its amount; a third (Cramer) makes it vary with the square root of the amount. It is thus attempted to prove that gambling must be a loss to *each* party, when it is fairly conducted. Take, for instance, Buffon's supposition, and suppose two persons each with two pounds, and they stake one. He who gains, gains one-third of his subsequent fortune; he who loses, loses one-half of his original fortune (this seems a somewhat variable standard by which to measure), therefore there is a total loss between them of $\frac{1}{2} - \frac{1}{3}$ or $\frac{1}{6}$. In fact, Buffon declares (*Arithmétique morale* § 13) that since this fraction must be lost, and it is an even chance which of them is the actual loser, it comes to exactly the same thing whether two persons thus stake half their fortune in an even wager, or whether they each throw one-twelfth of it into the sea.

The utmost that can be said for such Benthamic attempts at evaluating the elements of human welfare is that they have a foundation of truth in them; that in fact any one of them is much more nearly, and more generally, true than what might, loosely speaking, be called its opposite would be. But it appears to me that such large and important reservations and corrections have to be made, that the only reasonable and scientific course is to confine ourselves, wherever wealth is risked, to the so-called 'mathematical expectation,' and to leave the application of this to individual judgment and taste. This mathematical expectation is the product of the amount to be gained (or lost) and the chance of gaining (or losing) it; that is, it is the average amount which would be gained or lost in the long run by a repetition of similar ventures. We are prepared, in accordance with the usual mathematical principles, to assign the value of this expectation, under given circumstances, but we then leave it to the choice of the individual agents to say whether their tastes and position are such as to induce them to run the risk.

---

[1] It must be admitted that they have not been accepted so much on their own merits, as because they seemed to their proposers the only possible means of reconciling the conclusions of the Petersburg problem with the dictates of common sense.

§ 13. With regard to the practice of gambling, we have already remarked that its existence, especially on the enormous scale which it has commonly presented, is a partial confutation of any such doctrines as those mentioned in the last section; for the attempt to prove that it is a mere blunder and folly cannot but be considered as partaking somewhat of ingenious sophistry. Individuals may show any amount of short-sightedness, but the bulk of mankind must surely be credited with a measure of shrewdness sufficient to detect wherein pleasure is to be found. We have frequently had to touch on these questions in the course of this work, and have generally insisted that their consideration was alien to the strict science of Probability; but the nature of the special subject of this chapter must be our excuse for now devoting a few pages to it. There is the more reason for doing so since, although the motives and general consequences of gambling do not belong to Probability, problems arising out of pure games of chance have always formed a large part of the science.

It has already been pointed out that the skilful and persistent gambler must be regarded simply in the light of one who is following his trade, and who is probably keenly aware of its general progress and issue. The unskilful, but equally persistent, may often be considered as with almost equal consciousness following an amusement, and one which we must admit is not more expensive, frivolous, and useless, than the amusements of thousands of other loungers about similar gardens and *Kursaals* elsewhere. But may there not be cases in which the common rule, that great gains do not bring great satisfaction, may fail? Commerce and trade have much of the gambling element in them, and the maxim upon which thousands who follow them continually act, is that there are certain stages, so to say, the successive attainment of any of which will open new and great ranges of enjoyment. But for illustration, take a case of pure chance. Suppose a country overpopulated with labourers; might it not be advisable for the whole body of them to club together to enable a minority to emigrate—advisable, that is, not merely for those who went but for those who stayed behind as well? And how would the case be altered if they 'gambled' in order to decide to whom the preference should be given? They might fairly argue that the fun and excitement of the game was so much extra enjoyment. It appears to me that plenty of analogous examples, of a serious kind, might be suggested, in trade and elsewhere, in which men deliberately try for a large stake, from the belief that the pleasures contingent upon gain may be greater than the pains contingent upon loss. If one is for a moment to occupy the seat of the moralist, it seems a sounder plan not to lay down dogmatically a universal rule which the common sense of mankind feels to have many exceptions; but to point out general tendencies, and to condemn the practice of gambling rather upon public grounds of frivolity and mischief, which apply equally to many other practices, than upon the questionable ground that it is a shortsighted neglect of self-interest.

§ 14. So with Insurance. There is much to be said for it, probably, in a majority of cases; but we must not venture to offer any proof that it is a resource obligatory upon, or indeed advisable for, all men. We simply undertake to explain the principles in accordance with which men may commute contingent great losses for certain small ones; but we had better leave the final decision, whether or not they will do this, to their own taste and circumstances. Those who have a bulk of acquired property in addition to that which they have embarked in a risky business, will so far feel less inducement to guard against a loss which in their case assumes less serious proportions. Again, those whose business is on a very large scale, provided always that it is not subject to any common or simultaneous risk, may find themselves naturally placed in quite as good a situation as that which many insurance societies could confer upon them. There does not, for instance, seem to be any reason why the owners of a large shipping business should insure, at least in peace time, and it is, I believe, the fact that our great steam-packet companies do not do so. The same remark would generally apply to the case of the owners of much house property scattered over different parts of a town.

But apart from such cases, in which Insurance is rendered obviously unnecessary, it is still open to any one to maintain that he personally prefers the risk. It may be the case, as has already been pointed out, that in some countries, or under certain circumstances, a well-grounded conviction may exist that those who choose to work will always be able to take a fresh start, and to make their way again; and that this, combined with a stimulating sense of excitement and independence, may make it distinctly disadvantageous for some persons to submit to the constant though small burden of paying premiums.

§ 15. An ingenious attempt has been made by Mr Whitworth to prove that gambling is disadvantageous, without requiring us to resort to any such assumptions as those mentioned in § 12. When two persons play against each other one of the two must be ruined sooner or later, even though the game be a fair one, supposing that they go on playing long enough; the one with the smaller income having of course the worst chance of being the lucky survivor. If one of them has a finite, and the other an infinite income, it must clearly be the former who will be the sufferer. It is then maintained that this is in fact every individual gambler's position, "no one is restricted to gambling with one single opponent; the speculator deals with the public at large, with a world whose resources are practically unlimited. There is a prospect that his operations may terminate to his own disadvantage, through his having nothing more to stake; but there is no prospect that it will terminate to his advantage through the exhaustion of the resources of the world. Every one who gambles is carrying on an unequal warfare: he is ranged with a restricted capital against an adversary whose means are infinite[1]."

In the above argument it is surely overlooked that the adversaries against whom he plays are not one body with a common purse, like the bank in a gambling establishment. Each of these adversaries is in exactly the same position as he himself is, and a precisely similar proof might be employed to show that each of them must be eventually ruined, which is of course a reduction to absurdity. Gambling can only transfer money from one player to another, and therefore none of it can be actually lost.

---

[1] *Choice and Chance*, Ed. II. p. 208.

§ 16. What really becomes of the money, when they play to extremity, is not difficult to see. First suppose a limited number of players. If they go on long enough, the money will at last all find its way into the pocket of some one of their number. If their fortunes were originally equal, each stands the same chance of being the lucky survivor; in which case we cannot assert, on any numerical grounds, that the prospect of the play is disadvantageous to any one of them. If their fortunes were unequal, the one who had the largest sum to begin with can be shown to have the best chance, according to some assignable law, of being left the final winner; in which case it must be just as advantageous for him, as it was disadvantageous for his less wealthy competitors.

When, instead of a limited number of players, we suppose an unlimited number, each as he is ruined retiring from the table and letting another come in, the results are more complicated, but their general tendency can be readily distinguished. If we supposed that no one retired except when he was ruined, we should have a state of things in which all the old players were growing gradually richer. In this case the prospect before the new comers would steadily grow worse and worse, for their chance of winning against such rich opponents would be exceedingly small. But as this is an unreasonable supposition, we ought rather to assume that not only do the ruined victims retire, but also that those who have gained fortunes of a certain amount retire also, so that the aggregate and average wealth of the gambling body remains pretty steady. What chance any given player has of being ruined, and how long he may expect to hold out before being ruined, will depend of course upon the initial incomes of the players, the rules of the game, the stakes for which they play, and other similar considerations. But it is perfectly clear that for all that is lost by one, a precisely equal sum must be gained by others, and that therefore any particular gambler can only be cautioned beforehand that his conduct is not to be recommended, by appealing to some such suppositions as those already mentioned in a former section.

§ 17. As an additional justification of this view the reader may observe that the state of things in the last example is one which, expressed in somewhat different language and with a slight alteration of circumstances, is being incessantly carried on upon a gigantic scale upon every side of us. Call it the competition of merchants and traders and others in a commercial country, and the general results are familiar enough. It is true that in so far as skill comes into the question, they are not properly gamblers; but in so far as chance and risk do, they may be fairly so termed, and in many branches of business this must necessarily be the case to a very considerable extent. It is true again, that since they are productive labourers (at any rate some of them are) they are not in exactly the same position as those who are simply transferring wealth from one to another without adding to its amount. But to obviate this latter objection it would only be necessary to suppose that our party of gamblers, instead of starting with a fixed sum, were in possession of funds which gradually increased from extraneous sources. Whenever business is carried on in a reckless way, the comparison is on general grounds fair enough. In each case alike we find some retiring ruined, and some making their fortunes; and in each case alike also the chances, *cœteris paribus*, lie with those who have the largest fortunes. Every one is, in a sense, struggling against the collective commercial world, but since every one of his competitors is doing the same, we clearly could not caution any of them (except indeed the poorer ones) that their efforts must finally end in disadvantage.

CHAPTER XVI.

THE APPLICATION OF PROBABILITY TO TESTIMONY.

§ 1. On the principles which have been adopted and adhered to in this work, it will easily be seen that it becomes very questionable whether several classes of problems which may seem to have acquired a prescriptive right to admission, will not have to be excluded from the science of Probability. The most important, perhaps, of these refer to what is commonly called the credibility of testimony, estimated either at first hand and directly, or as influencing a juryman, and so combined with his sagacity and trustworthiness, Almost every treatise upon the science contains a discussion of the principles according to which credit is to be attached to combinations of the reports of witnesses of various degrees of trustworthiness, or the verdicts of juries consisting of larger or smaller numbers. A great modern mathematician, Poisson, has written an elaborate treatise expressly upon this subject; whilst a considerable portion of the works of Laplace, De Morgan, and others, is devoted to an examination of similar enquiries. It would be presumptuous to differ from such authorities as these, except upon the strongest grounds; but I confess that the extraordinary amount of ingenuity, thought, and mathematical ability which have been devoted to these problems, considered as questions in Probability, fails to convince me that they ever ought to have been so considered. The following are the principal grounds for this opinion.

§ 2. It will be remembered that in the course of the chapter on Induction we entered into a detailed investigation of the process demanded of us when, instead of the appropriate propositions from which the inference was to b made being set before us, the *individual* presented himsel and the task was imposed upon us of selecting the requisit groups or series to which to refer him. In other words, instead of calculating the chance of an event from determinat conditions of frequency of its occurrence (these being eithe obtained by direct experience, or deductively inferred) w have to *select* the conditions of frequency out of a pluralit of more or less suitable ones. When the problem is presented to us at such a stage as this, we may of course assum that the preliminary process of obtaining the statistic which are extended into the proportional propositions ha been already performed; we may suppose therefore that w are already in possession of a quantity of such proposition our principal remaining doubt being as to which of the we should then employ. This selection was shewn to b to a certain extent arbitrary; for, owing to the fact of th individual possessing a large number of different propertie he became in consequence a member of different series o groups, which might present different averages. We mus now examine, somewhat more fully than we did befor the practical conditions under which any difficulty arisin from this source ceases to be of importance.

§ 3. One condition of this kind is very simple and obvious. It is that the different statistics with which we are presented should not in reality offer materially different results. If, for instance, we were enquiring into the probability of a man aged forty dying within the year, we might if we pleased take into account the fact of his having red hair, or his having been born in a certain county or town. Each of these circumstances would serve to specialize the individual, and therefore to restrict the limits of the statistics which were applicable to his case. But the consideration of such qualities as these would either leave the average precisely as it was, or produce such an unimportant alteration in it as no one would think of taking into account. Though we could hardly say with certainty of any conceivable characteristic that it has absolutely no bearing on the result, we may still feel very confident that the bearing of such characteristics as these is utterly insignificant. Of course in the extreme case of the things most perfectly suited to the Calculus of Probability, viz. games of pure chance, these subsidiary characteristics are absolutely irrelevant. Any further particulars about the characteristics of the cards in a really fair pack, beyond those which are familiar to all the players, would convey literally no information whatever about the result.

Or again; although the different sets of statistics may not as above give almost identical results, yet they may do what practically comes to very much the same thing, that is, arrange themselves into a small number of groups, all of the statistics in any one group practically coinciding in their results. If for example a consumptive man desired to insure his life, there would be a marked difference in the statistics according as we took his peculiar state of health into account or not. We should here have two sets of statistics, so clearly marked off from one another that they might almost rank with the distinctions of natural kinds, and which would in consequence offer decidedly different results. If we were to specialize still further, by taking into account insignificant qualities like those mentioned in the last paragraph, we might indeed get more limited sets of statistics applicabl to persons still more closely resembling the individual i question, but these would not differ sufficiently in thei results to make it worth our while to do so. In other words, the different propositions which are applicable to the case in point arrange themselves into a limited number of groups, which, and which only, need be taken into account; whence the range of choice amongst them is very much diminished in practice.

§ 4. It may serve to make the foregoing remarks clearer to express them under a slightly different form. It was shewn in the first two chapters that, in the enquiries to which Probability introduces us, we are concerned with a series or indefinitely extensive class which is fixed by the presence of permanent attributes, the individuals of it being differenced (and thence a sub-class created) by the presence or absence of certain variable attributes. The conditions mentioned above are equivalent to asserting that these classes must be easily distinguishable; in other words, since the class is distinguished by means of certain attributes, this particular group of attributes must either be confined to it; or, if it be found elsewhere, must exist there in easily distinguishable degrees or in different combinations.

§ 5. The reasons for the conditions above described ar not difficult to detect. Where these conditions exist th process of selecting a series or class to which to refer an individual is very simple, and the selection is, for the particular purposes of inference, final. The process is simple for there being but a few classes, and these defined b easily distinguishable attributes, all we have to do in an particular case is to ascertain whether these attributes exist a proceeding which ought not in general to offer any difficulty. The selection also is final; for though the individua possesses many other attributes which, or the statistics appropriate to which, we may gradually come to recognize, these will not affect the result to any appreciable degree. It is assumed, as above described, that the consideration of these minor qualities does not materially disturb the statistics. We do not therefore trouble ourselves about their existence when we have once determined the data by which we mean to judge. In any case of insurance, for example, the question we have to decide is of the very simple kind; Is *A. B.* a man of a certain age? If so one in fifty in his circumstances die in the course of the year. If any further questions have to be decided they would be of the following description. Is *A. B.* a healthy man? Does he follow a dangerous trade? But here too the classes in question are but few, and the limits by which they are bounded are tolerably precise; so that the reference of an individual to one or other of them is easy. And when we have once chosen our class we remain untroubled by any further considerations; for since no other statistics are supposed to offer a materially different average, we have no occasion to take account of any other properties than those already noticed.

The case of games of chance, already referred to, offers of course an instance of these conditions in an almost ideal state of perfection; the same circumstances which fit them so eminently for the purposes of fair gambling, fitting them equally to become examples in Probability. When a die is to be thrown, all persons alike stand on precisely the same footing of knowledge and of ignorance about the result; the only data to which any one could possibly appeal ig that each face turns up on an average once in six es. From the standing point of the pure logician, we ht indeed divide dice into innumerable classes turning upon such considerations as the material of which they were made, the place at which they were bought, the nature of the box from which they were thrown, and so on, just as easily as we might divide them according to the number of sides which they possess. But the obvious fact that the introduction of the former class of considerations produces no effect whatever upon our conjecture about the result, whilst the latter produces a most vitally important effect, induces every one to reject the former subdivision as needlessly particular, and to insist upon the latter as all-important.

§ 6. Let us now examine how far the above conditions are fulfilled in the case of problems which discuss what is called the credibility of testimony. The following would be a fair specimen of one of the elementary enquiries out of which these problems are composed;—Here is a statement made by a witness who lies once in ten times, what am I to conclude about its truth? Objections might fairly be raised against the possibility of thus assigning a man his place upon a graduated scale of mendacity. This however we will pass over, and will assume that the witness goes about the world bearing stamped somehow on his face the appropriate class to which he belongs, and consequently, the degree of credit to which he has a claim on such general grounds. But there are other and even stronger reasons against the admissibility of this class of problems.

§ 7. That which has been described in the previous sections as the 'individual' which had to be assigned to an appropriate class or series of statistics is, of course, in this case, *a statement.* In the particular instance in question this individual statement is already assigned to a class, that namely of statements made by a witness of a given degree of veracity; but it is clearly optional with us whether or not we choose to confine our attention to this class in forming our judgment; at least it would be optional whenever we were practically called on to form an opinion. But in the case of this statement, as in that of the mortality of the man whose insurance we were discussing, there are a multitude of other properties observable, besides the one which is supposed to mark the given class. Just as in the latter there were (besides his age), the place of his birth, the nature of his occupation, and so on; so in the former there are (besides its being a statement by a certain kind of witness), the fact of its being uttered at a certain time and place and under certain circumstances. At the time the statement is made all these qualities or attributes of the statement are present to us, and we clearly have a right to take into account as many of them as we please. Now the question at present before us seems to be simply this;—Are the considerations, which we might thus introduce, as immaterial to the result in the case of the truth of a statement of a witness, as the corresponding considerations are in the case of the insurance of a life? There can surely be no hesitation in the reply to such a question. Under ordinary circumstances we soon know all that we can know about the conditions which determine us in judging of the prospect of a man's death, and we therefore rest content with general statistics of mortality; but no one who heard a witness speak would think of simply appealing to his figure of veracity, even supposing that this had been authoritatively communicated to us. The circumstances under which the statement is made instead of being insignificant, are of overwhelming importance. The appearance of the witness, the tone of his voice, the fact of his having objects to gain, together with a countless multitude of other circumstances which would gradually come to light as we reflect upon the matter, would make any sensible man utterly discard the assigned average from his consideration. He would, in fact, no more think of judging in this way than he would of appealing to the Carlisle or Northampton tables of mortality to determine the probable length of life of a soldier who was already in the midst of a battle.

§ 8. It cannot be replied that under these circumstances we still refer the witness to a class, and judge of his veracity by an average of a more limited kind; that we infer, for example, that of men who look and act like him under such circumstances, a much larger proportion, say nine-tenths, are found to lie. There is no appeal to a class in this way at all, there is no immediate reference to statistics of any kind whatever; at least none which we are conscious of using at the time, or to which we should think of resorting for justification afterwards. The entire decision seems to depend upon the quickness of the observer's senses and of his apprehension generally.

Statistics about the veracity of witnesses seem in fact to be permanently as inappropriate as all other statistics occasionally may be. We may know accurately the percentage of recoveries after amputation of the leg; but what surgeon would think of forming his judgment solely by such tables when he had a case before him? We need not deny, of course, that the opinion he might form about the patient's prospects of recovery might ultimately rest upon the proportions of deaths and recoveries he might have previously witnessed. But if this were the case, these data are lying, as one may say, obscurely in the background. He does not appeal to them directly and immediately in forming his judgment. There has been a far more important intermediate process of apprehension and estimation of what is essential to the case and what is not. Sharp senses, memory, judgment, and practical sagacity have had to be called into play, and there is not therefore the same direct conscious and sole appeal to statistics that there was before. The surgeon may have in his mind two or three instances in which the operation performed was equally severe, but in which the patient's constitution was different; the latter element therefore has to be properly allowed for. There may be other instances in which the constitution was similar, but the operation more severe; and so on. Hence, although the ultimate appeal may be to the statistics, it is not so directly; their value has to be estimated through the somewhat hazy medium of our judgment and memory, which places them under a very different aspect.

§ 9. The reader will have a good popular illustration of the nature of the difficulty which we have been considering, if he will recall to mind any dispute which he may have heard or taken part in, in which there was an appeal made to the analogy of cases similar to that in dispute. We must naturally select our example from somewhat complicated concrete instances. Suppose, for instance, that it had been the civil war in America. *A* thinks that the North will win because the party which is numerically inferior generally loses. (The appeal here, it should be observed, though not precisely statistical, is still roughly so: the failure occurs 'generally;' in Probability it would be properly assigned in a numerical proportion. But it is an appeal of a fundamentally similar character, and the nature of the argument from it is the same.) *B* retorts that a numerically inferior party when spread over a vast country generally is not beaten. *A* urges that slavery is generally a cause of weakness; not when there is a good feeling between the slaves and their masters, answers *B*. And so on *ad infinitum*. The reason why no settlement can thus be come to, is, I apprehend, the one given above. There is not here any system of natural classification universally recognized, and appealed to as final, so that there may be general agreement as to the class, by the statistics appropriate to which each party is ready to stand or fall. The particular circumstances of the case which may from time to time come into notice, are here of extreme importance from the marked alterations which they produce upon the averages. No one could by any possibility get up such a dispute if the question were whether a coming child were likely to be a boy or a girl.

Any one who knows anything of the game of whist may supply an apposite example of the distinction here insisted on, by recalling to mind the alteration in the nature of our inferences which shows itself as the game progresses. At the commencement of the game our sole appeal is rightfully made to the theory of Probability. All the rules upon which each player acts, and therefore upon which he infers that the others will act, rest upon the observed frequency (or rather upon the frequency which calculation assures us will be observed) with which such and such combinations of cards are found to occur. Why are we told, if we have more than four trumps, to lead them out at once? Because we are convinced, on pure grounds of probability, capable of being stated in the strictest statistical form, that in a majority of instances we shall draw our opponent's trumps, and therefore be left with the command. Similarly with every other rule which is recognized in the early part of the play.

But as the play progresses all this is changed, and towards its conclusion there is but little reliance upon any rules which either we or others could trace up to statistical frequency of occurrence, observed or inferred. A multitude of other considerations have come in; we begin to be influenced partly by our knowledge of the character and practice of our partner and opponents; partly by a rapid combination of a multitude of judgments, the grounds of which we could hardly realize or describe at the time and which have been entirely forgotten since. That is, the particular combination of cards, now before us, does not readily fall into any well-marked class to which alone it can reasonably be referred by every one who has the facts before him.

§ 10. A criticism somewhat resembling the above has been given by Mr Mill (*Logic*, Bk. III. Chap. xviii. § 3) upon the applicability of the theory of Probability to the credibility of witnesses. But he has added other reasons which do not appear to me to be equally valid; he says "common sense would dictate that it is impossible to strike a general average of the veracity, and other qualifications for true testimony, of mankind or any class of them; and if it were possible, such an average would be no guide, the credibility of almost every witness being either below or above the average." The latter objection would however apply with equal force to estimating the length of a man's life from tables of mortality; for the credibility of different witnesses can scarcely have a wider range of variation than the length of different lives. If statistics of credibility could be obtained, and could be conveniently appealed to when they were obtained, they might furnish us in the long run with as accurate inferences as any other statistics of the same general description, the individual variations of excess and defect being at length gradually neutralised. These statistics would however in practice naturally and rightly be neglected, because there can hardly fail to be circumstances in each individual statement which would more appropriately refer it to some new class depending on different statistics, and affording a far better chance of being right in that particular case. In most instances of the kind in question, indeed, such a change is thus produced in the mode of formation of our opinion, that, as already pointed out, the mental operation ceas be in any proper sense founded on appeal to statistics[1].

The reasons given above seem to me to tell str against the propriety of making testimony and its credil subjects of Probability. It is not denied, of course, th may if we please propose questions in this form, and solve them by the theory. When we do so however, opinions about real witnesses are not stated or answer such a way, we can scarcely regard the expression, 'a wi who lies once in ten times,' as being anything else tl sort of synonym for 'a bag yielding a black ball once ir times,' introduced into the discussion for the sake of variety. There are no restrictions whatever upon the of inventing examples for the sake of illustration, or the language in which we may express them. We mig we choose, assume that two geometrical theorems eacl once in ten times, and then determine the chance of a tion being correct, which they both agree in supporting.

§ 11. The Chance problems which are concerned wit timony are not altogether confined to such instances as hitherto referred to. Though we must, as it appears t reject all attempts to estimate the credibility of any ticular witness, or to refer him to any assigned cla respect of his trustworthiness, and consequently aband insoluble any of the numerous problems which start from such data as 'a witness who is wrong once in ten times,' yet it does not follow that testimony may not to a slight extent be treated by our science in a somewhat different manner. We may be quite unable to estimate, except in the roughest possible way, the veracity of any particular witness, and yet it may be possible to form some kind of opinion upon the veracity of certain classes of witnesses; to say, for instance, that Europeans are superior in this way to Orientals. So we might attempt to explain why, and to what extent, an opinion in which the judgments of ten persons, say jurors, concur, is superior to one in which five only concur. Something may also be done towards laying down the principles in accordance with which we are to decide whether, and why, extraordinary stories deserve less credence than ordinary ones, even if we cannot arrive at any precise and definite decision upon the point. This last question is further discussed in the course of the next chapter.

---

[1] It may be remarked also that there is another reason which tends to dissuade us from appealing to principles of Probability in the majority of the cases where testimony has to be estimated. It often, perhaps usually happens, that we are not absolutely forced to come to a decision; at least so far as the acquitting of an accused person may be considered as avoiding a d It may be of much greater i ance to us to attain not merel on the average, but truth i individual instance, so that rather not form an opinion than form one of which we c say in its justification that tend to lead us right in tl run.

The change of view in accordance with which it follows that questions of the kind just mentioned need not be entirely rejected from scientific consideration, presents itself in other directions also. It has, for instance, been already pointed out that the individual characteristics of any sick man's disease would be quite sufficiently important in most cases to prevent any surgeon from judging about his recovery by a genuine and direct appeal to statistics, however such considerations might indirectly operate upon his judgment. But if an opinion had to be formed about a considerable number of cases, say in a large hospital, statistics might again come prominently into play, and be rightly recognized as the principal source of appeal. We should feel able to compare one hospital, or one method of treatment, with another. The ground of the difference is obvious. It arises from the fact that the characteristics of the individuals, which made us so ready to desert the average when we had to judge of them separately, do not produce the same disturbance when we have to judge about a group of cases. The averages then become the most secure and available ground on which to form an opinion, and therefore Probability again becomes applicable.

But although some resort to Probability may be admitted in such cases as these, it nevertheless does not appear to me that they can ever be regarded as particularly appropriate examples to illustrate the methods and resources of the theory. Indeed it is scarcely possible to resist the conviction that the refinements of mathematical calculation have here been pushed to lengths utterly unjustifiable, when we bear in mind the impossibility of obtaining any corresponding degree of accuracy and precision in the data from which we have to start. To raise but one very obvious objection. Perfect independence amongst the witnesses or jurors is an almost necessary postulate. But where can this be secured? To say nothing of direct collusion, human beings are in almost all instances greatly under the influence of sympathy in forming their opinions[1]. This influence, under the various names of political bias, class prejudice, local feeling, and so on, always exists to a sufficient degree to induce a cautious person to make many of those individual corrections which we saw to be necessary when we were estimating the trustworthiness, in any given case, of a single witness; that is, they are sufficient to destroy much, if not all, of the confidence with which we resort to statistics and averages in forming our judgment. Since then this work is mainly devoted to explaining and establishing the general principles of the science of Probability, we may very fairly be excused from any further treatment of this subject, beyond the brief discussions which are given in the next chapter.

---

[1] Mr Monro has also called attention to a more subtle point which tells against the too common assumption of perfect independence by the mathematicians. "It is often remarked that the absence of this condition (independence) is a difficulty in these problems; but I doubt whether the difficulty is fully estimated. It is not merely that one man influences another, but that independently of this the judgment of one man ought to influence our expectation of the judgment of another; because human nature is so far uniform, that, from one man's adopting a particular opinion, there is some reason, whether the opinion is true or false, to expect that another will adopt the same under the same circumstances" (MS. communication).

CHAPTER XIV.

ON THE CREDIBILITY OF EXTRAORDINARY STORIES.

§ 1. It is now time to recur for fuller investigation to an enquiry which has been already briefly touched upon more than once; I mean the validity of testimony to establish, as it is frequently expressed, an otherwise improbable story. It will be remembered that in a previous chapter (the eleventh), we devoted some examination to an assertion by *Brown*, which seemed to be to some extent countenanced by Mill, that a great improbability before the proof might become but a very small improbability after the proof. In opposition to this it was pointed out that the different estimates which we undoubtedly formed of the credibility of the examples adduced, had nothing to do with the fact of the event being past or future, but arose from a very different cause; that the conception of the event which we entertain at the moment (which is all that is then and there actually present to us, and as to the correctness of which as a representation of facts we have to make up our minds) comes before us in very different ways. In one instance it was a mere guess of our own which we knew from statistics would be right in a certain proportion of cases; in the other instance it was the assertion of a witness, and therefore the appeal was not now to statistics of the event, but to the trustworthiness of the witness. The conception, or 'event' if we will so term it, had in fact passed out of the category of guesses (on statistical grounds), into that of assertions (most likely resting on some specific evidence), and would therefore be naturally regarded in a very different light.

§ 2. But it may seem as if this principle would lead us to somewhat startling conclusions. For, by transferring the appeal from the frequency with which the event occurs to the trustworthiness of the witness who makes the assertion, is it not implied that the probability or improbability of an assertion depends solely upon the veracity of the witness? If so, ought not any story whatever to be believed when it is asserted by a truthful person?

In order to settle this question we must look a little more closely into the circumstances under which such testimony is commonly presented to us. As it is of course necessary, for clearness of exposition, to take a numerical example, let us suppose that a given statement is made by a witness who, on the whole and in the long run, is right in what he says nine times out of ten[1]. Here then is an average given to us, an average veracity that is, which includes and is composed of all the particular statements which the witness has made or will make from time to time.

§ 3. Now it has been abundantly shown in a former chapter (Ch. VIII. §§ 14—32) that the mere fact of a particular average having been assigned, is no reason for our being forced invariably to adhere to it, even in those cases in which our most natural and appropriate ground of judgment is found in an appeal to statistics and averages. The general average may constantly have to be corrected in order to meet more accurately the circumstances of particular cases. In statistics of mortality, for instance, instead of resorting to the wider tables furnished by people in general at the given age, we often prefer the narrower tables furnished by men of a particular profession, abode, or mode of life. The reader may however be conveniently reminded here that in so doing we must not suppose that we are able, by any such device, in any special or peculiar way to secure truth. The general average, if persistently adhered to throughout a sufficiently wide and varied experience, would in the long run tend to give us the truth; all the advantage which the more special averages can secure for us is to give us the same tendency to the truth with fewer and slighter aberrations.

---

[1] Reasons were given in the last chapter against the propriety of applying the rules of Probability with any strictness to such examples as these. But although all approach to numerical accuracy is unattainable, we do undoubtedly recognize in ordinary life a distinction between the credibility of one witness and another; such a rough practical distinction will be quite sufficient for the purposes of this chapter. For convenience, and to illustrate the theory, the examples are best stated in a numerical form, but it is not intended thereby to imply that any such accuracy is really attainable in practice.

§ 4. Returning then to our witness, we know that, if our data about his average truthfulness are sound, we cannot really be *wrong*[1] in always relying upon it. That is to say, if we have a very great many statements from him upon all possible subjects, we may rest convinced that in nine out of ten of these he will tell us the truth, and that in the tenth case he will go wrong. This is nothing more than a matter of definition or consistency. But cannot we do better than thus rely upon his general average? Cannot we, in almost any given case, specialize it by attending to various characteristic circumstances in the nature of the statement which he makes; just as we specialize his prospects of mortality by attending to circumstances in his constitution or mode of life?

---

[1] It is not meant of course that we can regard such aberrations as these with the same complacency, in the long run, as we often can mere overstatements or understatements of numerical magnitude; or that they will be neutralized in time by equal errors of excess and defect. A direct assertion in place of a denial, or denial in place of assertion, can seldom be thus smoothed over in respect of its results by mere repetition. But a multitude of cases may readily be conceived in which we can allow a margin of, say, one-tenth for error, to meet the case of a witness who can only be depended on for nine-tenths of full veracity. If we know that, one time with another, he will mislead us once in ten times, we may be on our guard accordingly. Such security as this we may attain to in our dealings with him, if they are sufficiently numerous and varied.

Undoubtedly we may do this; and in any of the practical contingencies of life, supposing that we were at all guided by considerations of this nature, we should act very foolishly if we did not adopt some such plan. Two methods of thus correcting our average may be suggested, amongst others; one of them being that which practical sagacity would be most likely to employ, the other of a more subtle nature, and that which is almost universally adopted by writers on Probability. The former attempts to correct the average by the following considerations: instead of relying upon the witness' general average, we assign to it a sort of conjectural correction to meet the case before us, founded on our experience or observation; that is, we appeal to experience to establish that stories of such and such a kind are more or less likely to be true, as the case may be, than stories in general. The other proceeds upon a different and somewhat more philosophical plan. It is here endeavoured to show, by an analysis of the nature and number of the sources of error in the cases in question, that such and such kinds of stories must be more or less likely to be correctly reported.

§ 5. Before proceeding to a discussion of these methods a distinction must be pointed out to which writers upon the subject have not always attended, or at any rate have not generally sufficiently directed their readers' attention[1]. There are, broadly speaking, two different ways in which we may suppose testimony to be given. It may, in the first place, take the form of a reply to an alternative question, a question, that is, framed to be answered by a *yes* or *no*. Here, of course, the possible answers are mutually contradictory, so that if one of them is not correct the other must be so:— Has *A* happened, yes or no? The common mode of illustrating this kind of testimony numerically is by supposing a lottery with a prize and blanks, or a bag of balls of two colours only, the witness knowing that there are only two, or at any rate being confined to naming one or other of them. If they are black and white, and he errs when black is drawn, he must say 'white.' The reason for the prominence assigned to examples of this class is, probably, that they correspond to the very important case of verdicts of juries; juries being supposed to have nothing else to do than to say 'guilty' or 'not guilty.'

On the other hand, the testimony may take the form of a more original statement or piece of information. Instead of saying, Did *A* happen? we may ask, What happened? Here if the witness speaks truth he must be supposed, as before, to have but one way of doing so; for the occurrence of some specific event was of course contemplated. But if he errs he has many ways of going wrong, possibly an infinite number. Ordinarily however his possible false statements are assumed to be limited in number, as must generally be more or less the result in practice. This case is represented numerically by supposing the balls in the bag not to be of two colours only, but to be all distinct from one another; say by their being all successively numbered. It may of course be objected that a large number of the statements that are made in the world are not in any way answers to questions, either of the alternative or of the open kind. For instance, a man simply asserts that he has drawn the seven of spades from a pack of cards; and we do not know perhaps whether he had been asked 'Has that card been drawn?' or 'What card has been drawn?' or indeed whether he had been asked anything at all. Still more might this be so in the case of any ordinary historical statement.

---

[1] I must plead guilty to this charge myself, in the first edition of this work. The result was to make the treatment of this part of the subject obscure and imperfect, and in some respects erroneous.

This objection is quite to the point, and must be recognized as constituting an additional difficulty. All that we can do is to endeavour, as best we may, to ascertain, from the circumstances of the case, what number of alternatives the witness may be supposed to have had before him. When he simply testifies to some matter well known to be in dispute, and does not go much into detail, we may fairly consider that there were practically only the two alternatives before him of saying 'yes' or 'no.' When, on the other hand, he tells a story of a more original kind, or (what comes to much the same thing) goes into details, we must regard him as having a very wide comparative range of alternatives before him.

These two classes of examples, viz. that of the black and white balls, in which only one form of error is possible, and the numbered balls, in which there may be many forms of error, are the only two of which we need take notice. In practice it would seem that they may gradually merge into one another, according to the varying ways in which we choose to frame our question. Besides asking, Did you see *A* strike *B*? and, What did you see? we may introduce any number of intermediate leading questions, as, What did A do? What did he do to B? and so on. In this way we may gradually narrow the possible openings to wrong statement, and so approach to the direct alternative question. But it is clear that all these cases may be represented numerically by a supposed diminution in the number of the balls which are thus distinguished from one another.

§ 6. Of the two plans mentioned in § 4 we will begin with the latter, as it is the only methodical and scientific one which has been proposed. Suppose that there is a bag with 1000 balls, only one of which is white, the rest being all black. A ball is drawn at random, and our witness whose veracity is $\frac{9}{10}$ reports that the white ball was drawn. Take a great many of his statements upon this particular subject, say 10,000; that is, suppose that 10,000 balls having been successively drawn out of this bag, or bags of exactly the same kind, he makes his report in each case. His 10,000 statements being taken as a fair sample of his general average, we shall find, by supposition, that 9 out of every 10 of them are true and the remaining one false. What will be the nature of these false statements? Under the circumstances in question, he having only one way of going wrong, the answer is easy. In the 10,000 drawings the white ball would come out 10 times, and therefore be rightly asserted 9 times, whilst on the one of these occasions on which he goes wrong he has nothing to say but 'black.' So with the 9990 occasions on which black is drawn; he is right and says black on 8991 of them, and is wrong and therefore says white on 999 of them. On the whole, therefore, we conclude that out of every 1008 times on which he says that white is drawn he is wrong 999 times and right only 9 times. That is, his special veracity, as we may term it, for cases of this description, has been reduced from $\frac{9}{10}$ to $\frac{9}{1008}$. As it would commonly be expressed, the latter fraction represents the chance that this particular statement of his is true.

§ 7. We will now take the case in which the witness has many ways of going wrong, instead of merely one. Suppose that the balls were all numbered, from 1 to 1000, and the witness knows this fact. A ball is drawn, and he tells me that it was numbered 25, what are the odds that he is right? Proceeding as before, in 10,000 drawings this ball would be obtained 10 times, and correctly named 9 times. But on the 9990 occasions on which it was not drawn there would be a difference, for the witness has now many openings for error before him. It is, however, generally considered reasonable to assume that his errors will all take the form of announcing wrong numbers; and that, there being no apparent reason why he should choose one number rather than another, he will announce all the wrong ones equally often in turn[1]. Hence his 999 errors, instead of all leading him now back again to one spot, will be uniformly spread over as many distinct ways of going wrong. On one only of these occasions, therefore, will he mention 25 as having been drawn. It follows therefore that out of every 10 times that he names 25 he is right 9 times; so that in this case his average or general truthfulness applies equally well to the special case in point.

§ 8. With regard to the truth of these conclusions, it must of course be admitted that if we grant the validity of the assumptions about the limits within which the blundering or mendacity of the witness is confined, and the regularity with which his answers are disposed within those limits, the reasoning is perfectly sound. But are not these assumptions extremely arbitrary, that is, are not our lotteries and bags of balls rendered perfectly precise in many respects in which, in ordinary life, the conditions supposed to correspond to them are so utterly vague and uncertain that no such method of reasoning becomes practically available? Suppose that a person whom I have long known, and of whose measure of veracity and judgment I may be supposed therefore to have acquired some degree of knowledge, informs me that there is something to my advantage if I choose to go to certain trouble or expense in order to secure it. As regards the general veracity of the witness, then, there is no difficulty; we suppose that this is determined for us. But as regards his story, difficulty and vagueness emerge at every point. What is the number of balls in the bag here? What in fact are the nature and contents of the bag out of which we suppose the drawing to have been made? It does not seem to me that the materials for any rational judgment exist here. But if we are to get at any such amended figure of veracity as those attained in the above example, these questions must necessarily be answered with some degree of accuracy; for the whole point of the method consists in determining how often the event must be considered *not* to happen, and thence inferring how often the witness will be led wrongly to assert that it has happened.

---

[1] Such an example may be regarded as representing one of those intermediate cases considered at the end of § 5, in which the subject of testimony is limited by circumstances; being viz. a case in which the witness' errors are limited to announcing wrong numbers.

It is not of course denied that considerations of the kind in question have some influence upon our decision, but only that this influence could under any ordinary circumstances be submitted to numerical determination. We are doubtless liable to have information given to us that we have come in for some kind of fortune, for instance, when no such good luck has really befallen us; and this not once only but repeatedly. But who can give the faintest intimation of the nature and number of the occasions on which, a blank being thus really drawn, a prize will nevertheless be falsely announced? It appears to me therefore that results of any practical value can seldom, if ever, be looked for from this method of procedure.

§ 9. Our conclusion in the case of the lottery, or, what comes to the same thing, in the case of the bag with black and white balls, has been questioned or objected to[1] on the ground that it is contrary to all experience to suppose that the testimony of a moderately good witness could be so enormously depreciated under such circumstances. I should prefer to base the objection on the ground that experience scarcely ever presents such circumstances as those supposed; but if we postulate their existence the given conclusion seems correct enough. Assume that a man is merely required to say *yes* or *no;* assume also a group or succession of cases in which *no* should rightly be said very much oftener than *yes.* Then, assuming almost any general truthfulness of the witness, we may easily suppose the rightful occasions for denial to be so much the more frequent that a majority of his affirmative answers will actually occur as false 'noes' rather than as correct 'ayes.' This of course lowers the average value of his 'ayes,' and renders them comparatively untrustworthy.

§ 10. There is one particular case which has been regarded as a difficulty in the way of this treatment of the problem, but which seems to me to be a decided confirmation of it; always, be it understood, within the very narrow and artificial limits to which we must suppose ourselves to be confined. This is the case of a witness whose veracity is just one-half; that is, one who, when a mere *yes* or *no* is demanded of him, is as often wrong as right. In the case of any other assigned degree of veracity it is extremely difficult to get anything approaching to a confirmation from practical judgment and experience. We are not accustomed to estimate the merits of witnesses in this way, and hardly appreciate what is meant by his numerical degree of truthfulness. But as regards the man whose veracity is one-half, we are (as Mr Monro has very ingeniously suggested) only too well acquainted with such witnesses, though under a somewhat different name; for this is really nothing else than the case of a person confidently answering a question about a subject-matter of which he knows nothing, and can therefore only give a mere guess.

---

[1] Todhunter's *History*, p. 400. *Philosophical Magazine*, July, 1864.

Now in the case of the lottery with one prize, when the witness whose veracity is one-half tells us that we have gained the prize, we find on calculation that his testimony goes for absolutely nothing; the chances that we have got the prize are just the same as they would be if he had never opened his lips, viz. $\frac{1}{1000}$. But clearly this is what ought to be the result, for the witness who knows nothing about the matter leaves it exactly as he found it. He is indeed, in strictness, scarcely a witness at all; for the natural function of a witness is to examine the matter, and so to add confirmation, more or less, according to his judgment and probity, but at any rate to offer an improvement upon the mere guesser. If, however, we will give heed to his mere guess we are doing just the same thing as if we were to guess ourselves, in which case of course the odds that we are right are simply measured by the frequency of occurrence of the events.

We cannot quite so readily apply the same rule to the other case, namely to that of the numbered balls, for there the witness who is right every other time may really be a very fair, or even excellent, witness. If he has many ways of going wrong, and yet is right in half his statements, it is clear that he must have taken some degree of care, and cannot have merely guessed. In a case of *yes* or *no*, any one can be right every other time, but it is different where truth is single and error is manifold. To represent the case of a simply worthless witness when there were 1000 balls and the drawing of one assigned ball was in question, we should have to put his figure of veracity at $\frac{1}{1000}$. If this were done we should of course get a similar result.

§ 11. It deserves notice therefore that the figure of veracity, or fraction representing the general truthfulness of a witness, is in a way relative, not absolute; that is, it depends upon, and varies with, the general character of the answer which he is supposed to give. Two witnesses of equal intrinsic veracity and worth, one of whom confined himself to saying *yes* and *no*, whilst the other ventured to make more original assertions, would be represented by different fractions; the former having set himself a much easier task than the latter. The real caution and truthfulness of the witness are only one factor, therefore, in his actual figure of veracity; the other factor consists of the nature of his assertions, as just pointed out. The ordinary plan therefore, in such problems, of assigning an average truthfulness to the witness, and accepting this alike in the case of each of the two kinds of answers, though convenient, seems scarcely sound. This consideration would however be of much more importance were not the discussions upon the subject mainly concerned with only one description of answer, namely that of the 'yes or no' kind.

§ 12. So much for the philosophical way of treating such a problem. The way in which it would be taken in hand by those who had made no study of Probability is very different. It would, I apprehend, strike them as follows. They would say to themselves, Here is a story related by a witness who tells the truth, say, nine times out of ten. But it is a story of a kind which experience shows to be very generally made untruly, say 99 times out of 100. Having then these opposite inducements to belief, they would attempt in some way to strike a balance between them. Nothing in the nature of a strict rule could be given to enable them to decide how they might escape out of the difficulty. Probably, in so far as they did not judge at haphazard, they would be guided by still further resort to experience, or unconscious recollections of its previous teachings, in order to settle which of the two opposing inductions was better entitled to carry the day in the particular case before them. The reader will readily see that any general solution of the problem, when thus presented, is impossible. It is simply the now familiar case (Chap. VIII. §§ 14—32) of an individual which belongs equally to two distinct, or even, in respect of their characteristics, opposing classes. We cannot decide offhand to which of the two its characteristics most naturally and rightly refer it. A fresh induction is needed in order to settle this point.

§ 13. Rules have indeed been suggested by various writers in order to extricate us from the difficulty. The controversy about miracles has probably been the most fertile occasion for suggestions of this kind on one side or the other. It is to this controversy, I suppose, that the phrase is due, so often employed in discussions upon similar subjects, 'a contest of opposite improbabilities.' What is meant by such an expression is clearly this: that in forming a judgment upon the truth of certain assertions we may find that they are comprised in two very distinct classes, so that, according as we regarded them as belonging to one or the other of these distinct classes, our opinion as to their truth would be very different. Such an assertion belongs to one class, of course, by its being a statement of a particular witness, or kind of witness; it belongs to the other by its being a particular kind of story, one of what is called an improbable nature. Its belonging to the former class is so far favourable to its truth, its belonging to the latter is so far hostile to its truth. It seems to be assumed, in speaking of a contest of opposite improbabilities, that when these different sources of conviction co-exist together, they would each in some way retain their probative force so as to produce a contest, ending generally in a victory to one or other of them. Hume, for instance, speaks of our *deducting* one probability from the other, and apportioning our belief to the remainder[1]. Thomson, in his *Laws of Thought,* speaks of one probability as entirely superseding the other.

§ 14. It does not appear to me that the slightest philosophical value can be attached to any such rules as these. They doubtless may, and indeed will, hold in individual cases, but they cannot lay claim to any generality. Even the notion of a contest, as any necessary ingredient in the case, must be laid aside. For let us refer again to the way in which the perplexity arises, and we shall readily see, as has just been remarked, that it is nothing more than a particular exemplification of a difficulty which has already been recognized as incapable of solution by any general *à priori* method of treatment. All that we have before us is a statement. On this occasion it is made by a witness who lies, say, once in ten times in the long run; that is, who mostly tells the truth. But on the other hand, it is a statement which experience, derived from a variety of witnesses on various occasions, assures us is mostly false; stated numerically it is found, let us suppose, to be false 99 times in a hundred.

---

[1] "When therefore these two kinds of experience are contrary, we have nothing to do but subtract the one from the other, and embrace an opinion, either on one side or the other, with that assurance which arises from the remainder." (Essay on Miracles.)

Now, as was shown in the chapter on Induction, we are thus brought to a complete dead lock. Our science offers no principles by which we can form our opinion, or attempt to decide the matter one way or the other; for, as appeared, there are an indefinite number of conclusions which are all equally possible. For instance, all the witness' extraordinary assertions may be true, or they may all be false, or they may be divided into the true and the false in any proportion whatever. Having gone so far in our appeal to statistics as to recognize that the witness is generally right, but that his story is generally false, we cannot stop there. We ought to make still further appeal to experience, and ascertain how it stands with regard to his stories when they are of that particular nature: or rather, for this would be to make a needlessly narrow reference, how it stands with regard to stories of that kind when advanced by witnesses of his general character, position, sympathies, and so on[1].

§ 15. That extraordinary stories are in many cases, probably in a great majority of cases, less trustworthy than others must be fully admitted. That is, if we were to make two distinct classes of such stories respectively, we should find that the same witness, or similar witnesses, were proportionally more often wrong when asserting the former than when asserting the latter. But it does not by any means appear to me that this must always be the case. We may well conceive, for instance, that with some people the mere fact of the story being of a very unusual character may make them more careful in what they state, so as actually to add to their veracity. If this were so we might be ready to accept their extraordinary stories with even more readiness than their ordinary ones.

---

[1] Considerations of this kind have indeed been introduced into the mathematical treatment of the subject. The common algebraical solution of the problem in § 5 (to begin with the simplest case) is of course as follows. Let p be the antecedent probability of the event, and t the measure of the truthfulness of the witness; then the chance of his statement being true is $\frac{pt}{pt+(1-p)(1-t)}$. This supposes him to lie as much when the event does not happen as when it does. But we may meet the cases supposed in the text by assuming that t' is the measure of his veracity when the event does not happen, so that the above formula becomes $\frac{pt}{pt+(1-p)(1-t')}$. Here t' and t measure respectively his trustworthiness in usual and unusual events. As a formal solution this certainly meets the objections stated above in §§ 14 and 15. The determination however of t' would demand, as I have remarked, continually renewed appeal to experience. In any case the practical

Such a supposition as that just made does not seem to me by any means forced. Put such a case as this,—for though appeals to common impressions are no infallible test, they must in fairness be admitted to be of some value. Let us suppose that two persons, one of them a man of merely ordinary probity and intelligence, the other a philosophic naturalist, make a statement about some common events. We believe them both. Let them now each report some extraordinary *lusus naturæ* or monstrosity which they profess to have seen. Almost every one, we may presume, would receive the statement of the philosopher in this latter case as readily as in the former; whereas when the same story came from the unscientific observer it would be received with considerable hesitation. Whence arises the difference? From the conviction that the philosopher will be far more careful, and therefore to the full as accurate, in matters of this kind as in those of the most ordinary description, whereas

methods which would be adopted, if any plans of the kind indicated above were resorted to, seem to me to differ very much from that adopted by the mathematicians, in their spirit and plan.

with the other man we feel by no means the same confidence. Even if any one is not prepared to go this length, he will probably admit that the difference of credit which he would attach to the two kinds of story, respectively, when they came from the philosopher, would be much less than what it would be when they came from the other man.

§ 16. Whilst we are on this part of the subject, it must be pointed out that there is considerable ambiguity and consequent confusion about the use of the term 'an extraordinary story.' Within the province of pure Probability it ought to mean simply a story which asserts an *unusual* event. At least this is the view which has been adopted and maintained, it is hoped consistently, throughout this work. So long as we adhere to this sense we know precisely what we mean by the term. It has a purely objective reference; it simply connotes a very low degree of relative statistical frequency, actual or prospective. Out of a great number of events we suppose a selection of some particular kind to be contemplated, which occurs relatively very seldom, and this is termed an unusual or extraordinary event. It follows, as was abundantly shown in a former chapter, that owing to the rarity or unusualness of the event we are very little disposed to expect its occurrence in any given case. Our guess about it, in case we thus anticipated it, would very seldom be justified, and we are therefore apt to be much surprised when it does occur. This, I take it, is the only legitimate sense of 'extraordinary' so far as Probability is concerned.

But there is another and very different use of the word, which belongs to Induction, or rather to the science of evidence in general, more than to that limited portion of it termed Probability. In this sense the 'extraordinary,' and still more the 'improbable,' event is not merely one of extreme statistical rarity, which we could not expect to guess aright, but which on moderate evidence we may pretty readily accept; it is rather one which possesses, so to say, an actual evidence-resisting power. It may be something which almost always affects the credibility of the witness at the fountain-head, which makes, that is, his statements upon such a subject essentially inferior to those on other subjects. This is the case, for instance, with anything which excites his prejudices or passions or superstitions. In these cases it would seem unnatural to attempt to estimate the credibility of the witness by calculating (as in § 6) how often his errors would mislead us through his having been brought back to an affirmation instead of adhering correctly to a negation. We should rather be disposed to put our correction on the witness' average veracity at once. So again if the statement contradicts some recognized induction, or at least some empirical generalization. These cases, especially the latter, are broadly distinguished from the others, and nearly all the practical interest and importance of the controversy turn on such considerations as these. Unfortunately, however, we cannot afford space to give any full discussion of them here, as they do not properly belong to Probability.

§ 17. In true Probability, as has just been remarked, every event has its own definitely recognizable degree of frequency of occurrence. It may be excessively rare, rare to any extreme we like to postulate, but still every one who understands and admits the data upon which its occurrence depends will be able to appreciate within what range of experience it may be expected to present itself. We do not expect it in any individual case, nor within any brief range, but we do confidently expect it within an adequately extensive range. How therefore can miraculous stories be similarly taken account of, when the disputants, on one side at least, are not prepared to admit their actual occurrence anywhere or at any time? How can any arrangement of bags and balls, or other mechanical or numerical illustrations of unlikely events, be admitted as fairly illustrative of miraculous occurrences, or indeed of many of those which come under the designation of 'very extraordinary' or 'highly improbable'? Those who contest the occurrence of a particular miracle, as reported by this or that narrator, do not admit that miracles are to be confidently expected sooner or later. It is not a question as to whether what must happen sometimes has happened some particular time, and therefore no illustration of the kind can be regarded as apposite.

How unsuitable these merely rare events, however excessive their rarity may be, are as examples of miraculous events, will be evident from a single consideration. No one, I presume, who admitted the occasional occurrence of an exceedingly unusual combination, would be in much doubt if he considered that he had actually seen it himself[1]. On the other hand, few men of any really scientific turn would readily accept a miracle even if it appeared to happen under their very eyes. They might be staggered at the time, but they would probably soon come to discredit it afterwards, or so explain it away as to evacuate it of all that is meant by miraculous.

---

[1] Laplace, for instance (*Essai*, ed. 1825, p. 149), says that if we saw 100 dies (known of course to be fair ones) all give the same face, we should be bewildered at the time, and need confirmation from others, but that, after due examination, no one would feel obliged to postulate hallucination in the matter. But the chance of this occurrence is represented by a fraction whose numerator is 1, and denominator contains 77 figures, and is therefore utterly inappreciable by the imagination. It must be admitted, though, that there is something hypothetical about such an example, for we could not really know that the dies were fair with a confidence even distantly approaching such prodigious odds.

§ 18. Of the two distinct kinds of case mentioned in § 5 the first is of course taken as the fitting type of the report of an improbable event, or of any kind of wonder ranging upwards to the miraculous. As was pointed out, there seems much ambiguity and liability to confusion in this way of speaking; and I much prefer, in consequence, the plan of confining oneself as much as possible in Probability to such terms as 'rare' and 'unusual.' As a mere objective occurrence the No. 25 is just as unusual, that is, as extraordinary, as the single white ball or the single prize. It demands precisely as unusual a combination of conditions to bring it about. It occurs therefore as seldom, and is rightly as little expected. The reason why we are less inclined to accept the statement about the one white ball has no connection with any intrinsic incredibility in the event itself; it arises purely out of what may be called, by comparison, the accidental fact that from the nature of the testimony in that class of cases, such events are much more frequently wrongly asserted.

§ 19. It appears to me therefore, on the whole, that very little can be made of these problems of testimony in the way in which it is generally intended that they should be treated; that is, in obtaining specific rules for the estimation of the testimony under any given circumstances. Assuming that the veracity of the witness can be measured, we meet the real difficulty in the utter impossibility of determining the limits within which the failures of the event in question are to be considered to lie, and the degree of explicitness with which the witness is supposed to answer the enquiry addressed to him; both of these being characteristics of which it is absolutely necessary to have a numerical estimate before we can consider ourselves in possession of the requisite data.

Since therefore the practical resort of most persons, viz. that of putting a direct and immediate correction, of course of a somewhat conjectural nature, upon the general trustworthiness of the witness, by a consideration of the nature of the circumstances under which his statement is made, is essentially unscientific and irreducible to rule; it really seems to me that there is something to be said in favour of the simple plan of trusting in all cases alike to the witness' general veracity[1]. That is, whether his story is ordinary or extraordinary, we may resolve to put it on the same footing of credibility, provided of course that the event is fully recognized as one which does or may occasionally happen. It is true that we shall thus go constantly astray, and may do so to a great extent, so that if there were any rational and precise method of specializing his trustworthiness, according to the nature of his story, we should be on much firmer ground. But at least we may thus know what to expect on the average. Provided we have a sufficient number and variety of statements from him, and always take them at the same constant rate or degree of trustworthiness, we may succeed in balancing and correcting our conduct in the long run so as to avoid any ruinous error.

§ 20. A few words may now be added about the combination of testimony. No new principles are introduced here, though the consequent complication is naturally greater. Let us suppose two witnesses, the veracity of each being $\frac{9}{10}$. Now suppose 100 statements made by the pair; according to the plan of proceeding adopted before, we should have them both right 81 times, and both wrong once, in the remaining 18 cases one being right and the other wrong. But since they are both supposed to give the same account, what we have to compare together are the number of occasions on which they agree and are right, and the total number on which they agree whether right or wrong. The ratio of the former to the latter is the fraction which expresses the trustworthiness of their combination of testimony in the case in question.

---

[1] In the first edition this was stated, as it now seems to me, in decidedly too unqualified a manner. It must be remembered, however, that (as was shown in § 7) this plan is really the best theoretical one which can be adopted in certain cases.

In attempting to decide this point the only difficulty is in determining how often they will be found to agree when they are both wrong, for clearly they must agree when they are both right. This enquiry turns of course upon the number of ways in which they can succeed in going wrong. Suppose first the case of a simple *yes* or *no* (as in § 6), and take the same example, of a bag with 1000 balls, in which one only is white. Proceeding as before, we should find that out of 100,000 drawings (the number required in order to obtain a complete cycle of all possible occurrences, as well as of all possible reports about them) the two witnesses agree in a correct report of the appearance of white in 81, and agree in a wrong report of it in 999. The Probability therefore of the story when so attested is $\frac{81}{1080}$; the fact therefore of two such witnesses of equal veracity having concurred makes the report nearly 9 times as likely as when it rested upon the authority of only one of them.

§ 21. When however the witnesses have many ways of going wrong, the fact of their agreeing makes the report far more likely to be true. For instance, in the case of the 1000 numbered balls, it is very unlikely that when they both mistake the number they should (without collusion) happen to make the same misstatement. Whereas, in the last case, every combined misstatement necessarily led them both to the assertion that the event in question had happened, we should now find that only once in 999 × 999 times would they both be led to assert that *some given number* (say, as before, 25) had been drawn. The odds in favour of the event in fact now become $\frac{80919}{80920}$, which are enormously greater than when there was only one witness.

It appears therefore that when two, and of course still more when many, witnesses agree in a statement in a matter about which they might make many and various errors, the combination of their testimony adds enormously to the likelihood of the event; provided always that there is no chance of collusion. And in the extreme case of the opportunities for error being, as they well may, practically infinite in number, such combination would produce a practically perfect condition. But then this condition, viz. absence of collusion, very seldom can be secured. Practically our main source of error and suspicion is in the possible existence of some kind of collusion. Since we can seldom entirely get rid of this danger, and when it exists it can never be submitted to numerical calculation, it appears to me that combination of testimony, in regard to detailed accounts, is yet more unfitted for consideration in Probability than even that of single testimony.

§ 22. The impossibility of any adequate or even appropriate consideration of the credibility of miraculous stories by the rules of Probability has been already noticed in § 17. But, since the grounds of this impossibility are often very insufficiently appreciated, a few pages may conveniently be added here with a view to enforcing this point. If it be regarded as a digression, the importance of the subject and the persistency with which various writers have at one time or another attempted to treat it by the rules of our science must be the excuse for entering upon it.

A necessary preliminary will be to decide upon some definition of a miracle. It will, we may suppose, be admitted by most persons that in calling a miracle 'a suspension of a law of causation,' we are giving what, though it may not amount to an adequate definition, is at least true as a description. It is true, though it may not be the whole truth. Whatever else the miracle may be, this is its physical aspect, this is the point at which it comes into contact with the subject-matter of science. If it were not considered that any suspension of causation were involved, the event would be regarded merely as an ordinary one to which some special significance was attached, that is, as a type or symbol rather than a miracle. It is this aspect moreover of the miracle which is now exposed to the main brunt of the attack, and in support of which therefore the defence has generally been carried on. For these reasons we will commence with this view of it.

Now it is obvious that this, like most other definitions or descriptions, makes some assumption as to matters of fact, and involves something of a theory. The assumption clearly is, that laws of causation prevail universally, or almost universally, throughout nature, so that infractions of them are marked and exceptional. This assumption is made, but it does not appear that anything more than this is necessarily required; that is, there is nothing which need necessarily make us side with either of the two principal schools which are divided as to the nature of these laws of causation. The definition will serve equally well whether we understand by *law* nothing more than uniformity of antecedent and consequent, or whether we assert that there is some deeper and more mysterious tie between the events than mere sequence. The use of the term 'causation' in this minimum of signification is common to both schools, though the one might consider it inadequate; we may speak, therefore, of 'suspensions of causation' without committing ourselves to either.

§ 23. It should be observed that the aspect of the question suggested by this definition is one from which we can hardly escape. Attempts indeed have been sometimes made to avoid the necessity of any assumption as to the universal prevalence of law and order in nature, by defining a miracle from a different point of view. A miracle may be called, for instance, 'an immediate exertion of creative power,' 'a sign of a revelation,' or, still more vaguely, an 'extraordinary event.' But nothing would be gained by adopting any such definitions as these. However they might satisfy the theologian, the student of physical science would not rest content with them for a moment. He would at once assert his own belief, and that of other scientific men, in the existence of universal law, and enquire what was the connection of our definition with this doctrine. An answer would imperatively be demanded to the question, Does the miracle, as you have described it, imply an infraction of one of these laws, or does it not? And an answer must be given, unless indeed we reject his assumption by denying our belief in the existence of this universal law, in which case of course we put ourselves out of the pale of argument with him. The necessity of having to recognize this fact is growing upon men day by day, with the increased study of physical science. And since this aspect of the question has to be met some time or other, it is as well to place it in the front. The difficulty, in its scientific form, is of course a modern one, for the doctrine out of which it arises is modern. But it is only one instance, out of many that might be mentioned, in which the growth of some philosophical conception has gradually affected the nature of the dispute, and at last shifted the position of the battle-ground, in some discussion with which it might not at first have appeared to have any connection whatever.

§ 24. So far our path is plain. Up to this point disciples of very different schools may advance together; for in laying down the above doctrine we have carefully abstained from implying or admitting that it contains the whole truth. But from this point two paths branch out before us, paths as different from each other in their character, origin, and direction, as can well be conceived. As this enquiry is only a digression, we may confine ourselves to stating briefly what seem to be the characteristics of each, without attempting to give the arguments which might be used in their support.

(I.) On the one hand, we may assume that this principle of causation is the ultimate one. By so terming it, we do not mean that it is one from which we consciously start in our investigations, as we do from the axioms of geometry, but rather that it is the final result towards which we find ourselves drawn by a study of nature. Finding that, throughout the scope of our enquiries, event follows event in never-failing uniformity, and finding moreover (some might add) that this experience is supported or even demanded by a tendency or law of our nature (it does not matter here how we describe it), we may come to regard this as the one great principle on which all our enquiries should rest.

(II.) Or, on the other hand, we may admit a class of principles of a very different kind. Allowing that there is this uniformity so far as our experience extends, we may yet admit what I can see no other way of describing than by calling it a Superintending Providence, that is, a Scheme or Order, in reference to which Design may be predicated without using merely metaphorical language. To adopt an aptly chosen distinction, it is not to be understood as *overruling* events, but rather as *underlying* them. I repeat again, that it is not my present object to enter into any discussion as to whence we obtain this idea.

§ 25. It is not easy to see how any reflecting mind can fail, at the present time, to be subject to one or other of these prepossessions. This word 'prepossession' has, in common use, a bad signification, almost equivalent to prejudice, but I use it here because there seems no other which would express the meaning equally well. If the principles by which we judge were intuitively and immediately obvious, we might conceive any one to be able in a sort of way to divest himself of prepossessions. These principles could be stated, like the axioms on the first pages of Euclid, and any casual bystander would be able to state in a few minutes whether he adopted them or not. This gives the appearance of commencing absolutely from the beginning. But if first principles are only to be acquired by long and laborious reflection, such a demand as this is quite out of the question. Though acquired in the most unexceptionable way, such principles may, in reference to any particular problem, be called prepossessions; and if they are challenged or denied, all that any one can do is to repeat his conviction of their truth, and his belief that other persons by reflection will or should come to regard them as he does; he cannot compel the assent of his opponent.

§ 26. Now it is quite clear that according as we come to the discussion of any particular miracle or extraordinary story under one or other of these prepossessions, the question of its credibility will assume a very different aspect. It is sometimes strangely overlooked that although a difference about *facts* is one of the conditions of a *bonâ fide* argument, a difference which reaches to ultimate principles is fatal to all argument. The possibility of present conflict is banished in such a case as absolutely as that of future concord. A large amount of popular literature on the subject of miracles seems to labour under this hopeless defect. Arguments are stated and examined for and against the credibility of miraculous stories without the disputants appearing to have any adequate conception of the chasm which separates one side from the other.

§ 27. The following illustration may serve in some degree to show the sort of inconsistency of which we are speaking. A sailor reports that in some remote coral island of the Pacific, on which he had landed by himself, he had found a number of stones on the beach disposed in the exact form of a cross. Now if we conceive a debate to arise about the truth of his story, in which it is attempted to decide the matter simply by considerations about the validity of testimony, without introducing the question of the existence of inhabitants, and the nature of their customs, we shall have some notion of the unsatisfactory nature of many of the current arguments about miracles. All illustrations of this subject are imperfect, but a case like this, in which a supposed trace of human agency is detected interfering with the orderly sequence of other and non-intelligent natural causes, is as much to the point as any illustration can be. The thing omitted here from the discussion is clearly the one important thing. If we suppose that there is no inhabitant, we shall probably disbelieve the story, or consider it to be so grossly exaggerated as to have but little truth in it. If we suppose that there are inhabitants, the question is at once resolved into a different and somewhat higher one. The credibility of the witness is not the only element, but we should necessarily have to take into consideration the character of the supposed inhabitants, and the object of such an action on their part.

§ 28. Considerations of this character are doubtless often introduced into the discussion, but it appears to me that they are introduced to a very inadequate extent. It is often urged, after Paley, 'Once believe in a God, and miracles are not incredible.' Such an admission surely demands some modification and extension. It should rather be stated thus, Believe in a God whose working may be traced throughout the whole moral and physical world. It amounts, in fact, to this;—Admit that there may be a *design* which we can trace somehow or other in the course of things; admit that we are not wholly confined to tracing the connection of events, or following out their effects, but that we can form some idea, feeble and imperfect though it be, of a *scheme*[1]. Paley's advice sounds too much like saying, Admit that there are fairies, and we can account for our cups being cracked. The admission is not to be made in so off-hand a manner. To any one labouring under the difficulty we are speaking of, this belief in a God almost out of any constant relation to nature, whom we then imagine to occasionally manifest himself in a perhaps irregular manner, is altogether impossible. The only form under which belief in the Deity can gain entrance into his mind is as the controlling Spirit of an infinite and orderly system. In fact, it appears to me, paradoxical as the suggestion may appear, that it might even be more easy for a person thoroughly imbued with the spirit of Inductive science, though an atheist, to believe in a miracle which formed a part of a vast system, as the Christian miracles may be supposed to do, than for such a person, as a theist, to accept an isolated miracle. It must be repeated again, that we are not concerned at present with the origin of our belief in design or a Providential scheme. If any people can find it in a study of physical science alone, so much the better for them; but whether it be obtained thence, or from moral and metaphysical grounds, or from Revelation, we must possess it, or miracles at the present day will be hard of credit.

§ 29. It is therefore with great prudence that Hume, and others after him, have practically insisted on commencing with a discussion of the credibility of the single miracle, treating the question as though the Christian Revelation could be adequately regarded as a succession of such events. As well might one consider the living body to be represented by the aggregate of the limbs which compose it. What is to be complained of in so many popular discussions on the subject is the entire absence of any recognition of the different ground on which the attackers and defenders of miracles are so often really standing. Proofs and illustrations are produced in endless variety, which involving, as they almost all do in the mind of the disputants, on one side at least, the very principle of causation, the absence of which in the case in question they are intended to establish, they fail in the single essential point. To attempt to induce any one to disbelieve in the existence of physical causation, in a given instance, by means of illustrations which to him seem only additional examples of the principle in question, is like trying to make a dam, in order to stop the flow of a river, by shovelling in snow. Such illustrations are plentiful in times of controversy, but being in reality only modified forms of that which they are applied to counteract, they change their shape at their first contact with the disbeliever's mind, and only help to swell the flood which they were intended to check.

---

[1] The stress which Butler lays upon this notion of a scheme is, I think, one great merit of his *Analogy*.

§ 30. The bearing of the last few sections may be expressed as follows. Any one who believes that the moral and physical world form one great scheme need find no insuperable difficulty in accepting a Revelation which forms a portion of such a scheme, nor consequently in accepting the miracles, collectively and individually, which are connected with the Revelation. But if, on the other hand, we start with the Inductive principle of uniform causation, and then attempt (leaving the notion of Providential superintendence out of sight) to establish, first, such and such a miracle, and thence a Revelation; it is hard to see how, on such principles, in the present state of feeling about scientific evidence, any accumulation of testimony could do more than baffle and perplex the judgment at the time, and leave us finally in doubt.

CHAPTER XVIII.

ON STATISTICS AS APPLIED TO HUMAN ACTIONS.

§ 1. THROUGHOUT this Essay examples have been drawn almost indifferently both from purely physical phenomena and from those which are concerned directly with human actions; in the case of the latter, moreover, some of our examples refer to conduct which is purely voluntary, whilst in others the human will is but a remote cause of the effects described. It is now time to enter into a short enquiry as to how far it is right thus to put these various human actions, especially those of the more exclusively voluntary description, upon the same footing as the results of the seasons or the turning up of the faces of a die, and to subject them all alike to the same rules.

The question before us is, of course, but a limited portion of a far wider question which has been much debated of late years, namely, whether the general principles of what we have termed Phenomenalist or Material Logic are as applicable to the facts of society as they are generally admitted to be to those of inanimate nature. It is needless to say that nothing professing to be an adequate investigation of such a subject as this will be made in the present chapter; but the enquiry is one which must have been so often suggested in some of the previous chapters, that it cannot be altogether passed over here. In Probability we are concerned only with a limited portion of human conduct, for the nature of our science confines our attention to such actions as tend to show some uniformity when arranged in groups, or examined in succession. But inasmuch as the criticisms which will presently be offered apply equally to drawing inferences about human actions of almost any kind, it will be simpler to commence our enquiry under the latter or more general form.

§ 2. It has been already repeatedly stated that the standing-point occupied by the observer who is supposed to make the inferences we have been considering, is that in which he looks out on to things which are happening about him. He is supposed to observe coexistences and sequences of things around him, which he then proceeds to analyse and classify, and from which he draws what inferences he can. To retain such a standing-point consistently two conditions, amongst others, seem to be presupposed. These are (1) That the observer should leave the things which he is engaged in observing to work out their courses undisturbed by any interference on his own part. (2) That he should adhere consistently to the position of an observer, and not in imagination step down and take a place amongst the things which he observes. In the attempt to construct the Logic of Society, or Sociology as it is often termed, both of the above conditions seem to be often neglected. The neglect of the former is, I think, an inherent imperfection in any such science of human conduct; that of the latter is rather a fallacy into which loose thinkers are apt to fall. We will examine these conditions in turn.

§ 3. (I.) To say that the objects of any kind whose behaviour we are considering are to be left free from any interference on our own part, is to make a claim which is so obviously demanded, that the caution may seem unnecessary.

And it certainly is not needed in the case of most inferences about inanimate objects. Any person can see that to draw inferences about a thing, and then to introduce a disturbance into its conduct which was not contemplated when the inference was drawn, is to invalidate the results we have obtained. But when the inference is about the conduct of human beings it is often forgotten that in the inference itself, if published, we may have produced an unsuspected source of disturbance. In other words, if the results of our investigations be given in the form of statements as to what people are doing and what they will do, the moment these statements come before their notice the agents will be subject to a new motive which will produce a disturbance in the conduct which had been inferred. We may make what statements and criticisms we please about the *past* conduct of men, but directly we commit ourselves to any statements about the future, or, in other words, begin to make predictions, we lay ourselves open to the difficulty just mentioned. That predictions can be made seems to be held by most of those who have adopted the application of logic now under consideration. They do not, of course, claim to be able to foretell the particular actions of individuals, but they constantly assert that it is quite possible that we may some day be able to foretell general tendencies, and the results of the conduct of large masses of men.

§ 4. The following extracts from Mill's *Logic*, Bk. VI. ch. iii. § 2, will contain the best compendious description of these claims of Sociology. After referring to the condition in which astronomy once was, and in which the science of the tides now is, he describes in the following words the practical aims of Sociology, and the ideal perfection of the science from which we are precluded only by the imperfection of our faculties:—"The science of human nature is of this description. It falls far short of the standard of exactness now realized in Astronomy; but there is no reason that it should not be as much a science as Tidology is, or as Astronomy was when its calculations had only mastered the main phenomena, but not the perturbations.

"The phenomena with which this science is conversant being the thoughts, feelings, and actions of human beings, it would have attained the ideal perfection of a science if it enabled us to foretell how an individual would think, feel, or act, throughout life, with the same certainty with which astronomy enables us to predict the places and the occultations of the heavenly bodies."

§ 5. It will hardly be denied that there is the following distinct theoretical objection to the above illustration. The publication of the Nautical Almanack is not supposed to have the slightest effect upon the path of the planets, whereas the publication of any prediction about the conduct of human beings (unless it were kept out of their sight, or expressed in unintelligible language) almost certainly would have some effect. The existence of this distinction renders all such physical illustrations entirely inapplicable when we thus attempt to explain the way in which it is supposed that human conduct can be studied and foretold.

§ 6. It should be clearly understood that we need not be under any apprehension of getting involved in any Fate and Free-will controversy here; the difficulty before us does not arise out of the *foreknowledge*, but out of the *foretelling*, of what the agents are going to do. Assuming that the abstract possibility of foreseeing human conduct, alluded to in the extract above quoted, is quite compatible with our practical consciousness of freedom, it must be maintained that a difficulty of an entirely distinct character is introduced the moment we suppose that this conduct is foretold, or rather, if one may use the term, *forepublished.* After all the causes have been estimated which can affect the agent, with the single exception of the sociological publication which describes his conduct, we shall very possibly find that the result is subsequently falsified by the disturbing agency of this publication itself.

This disturbance, observe, is not of the nature of a mere complication of the result; it takes the form of introducing a distinct contradiction. Some particular action was going to be done, and was therefore announced; in consequence of the announcement that action is not done, but something else is done instead. But had this further consequence been foreseen (as we must, on our present assumption, suppose might have been the case) and allowed for, we still shall not find any escape from the difficulty. Were this all we had to take into account we should have nothing further to apprehend than a complication; but beyond all this there is the conflict between the final announcement and the conduct announced, which cannot be avoided. It must be repeated again, that it is not foreknowledge, but foretelling, that creates the difficulty; the observer, after he has made his announcement, or whilst he is making it, may be perfectly aware of the effect it will produce, and may even privately communicate the result to others, but once let him make it so public that it reaches the ears of those to whom it refers, and his work is undone. His position, in fact, is somewhat like that attributed to Jonah at Nineveh. Giving the prophet the fullest recognition of his power of foreseeing things as they would actually happen, we must yet admit that he labours under an inherent incapacity of publicly announcing them in that form. The city was going to be destroyed; Jonah announces this; in consequence the people repent and are spared. But had he foretold their repentance and escape, the repentance might never have taken place. He might, of course, make a hypothetical statement, so as to provide for either alternative, but a categorical statement is always in danger of causing its own falsification.

§ 7. The only reply, I think, can be, that although the above difficulty is a theoretic objection, the effects referred to will be so utterly insignificant that they can be neglected in practice. But it is surely very doubtful whether distinct statements about human beings can be expected to produce little or no effect upon their conduct. The magnitude of this disturbing effect would seem to depend in great part upon the nature of the particular announcement made, and the intelligence and readiness to believe of the agents referred to in it. If the announcement is concerned with matters of little importance, or with the conduct of persons who for any reason are not likely to take notice of what is said about them, then the considerations to which we have been referring might be neglected without serious error. We might calculate and publish as much as we please about the fate of any of the depressed classes at the bottom of the social scale, without any serious anxiety that our predictions and conclusions would in consequence be falsified, except in so far as others were interested in their fate. But we should soon find a considerable difference if we were to begin to discuss in this way the prospects of any persons who were likely to take an interest in our proceedings. It may be true that at present but little effect would be produced by any statements that we might publish about the future of society, because the possibility of making such statements is doubted; but if Sociology were ever to establish its claims, the effects produced in each case by its own disturbing agency would rise into real importance.

§ 8. The foregoing remarks apply principally to the case of a prediction being distinctly falsified owing to its statements being of a disagreeable character, but it must not be supposed that the difficulty is confined to such cases as these. The prediction may be equally falsified if it holds out an attractive prospect. There will not indeed be the same direct contradiction here, arising out of the agents abstaining from what was foretold, but if, in consequence of the announcement, they perform their actions more speedily or more effectually than they would otherwise have done, the prediction is still rendered incorrect. The conduct, as it is finally carried out, is not the consequence of the motives which had been taken into account only, but of these together with the additional motives introduced by the publication. The nature of the disturbance which would be thus produced, as dependent upon the character of the announcement made and the circumstances under which it was published, have never, I think, been properly taken account of, but they seem well worthy of examination.

An instance has already occurred (Ch. VIII. § 25) of the kind of practical difficulty which might arise from the cause now under discussion. The subject is far too wide for us to enter more fully into it here; I will only therefore call the reader's attention to the fact of the existence of this cause of disturbance and the consequent caution that any inferences from our satistics cannot be warranted, when we extend them into the future, unless under the condition that the persons whose conduct is described either are left in ignorance of the statistics, or, if they know them, are still uninfluenced by them.

§ 9. (II.) The remarks in the last few sections are intended to point out that that purely speculative and isolated position of the observer, which alone is tenable when we are laying down rules for a science of inference, is one which it is in certain cases practically impossible to maintain. With every wish to be nothing more than simple observers, we cannot always secure our isolation when we are describing the conduct of intelligent human beings, for we cannot always prevent them from being influenced by what we say. The criticism I have next to offer is of a very different kind. It refers not to the actual disturbance caused unintentionally by the observer's published inferences, but to an intentional hypothetical disturbance in the actions which form the subject of the inference. The possibility of such a disturbance being contemplated arises from the fact that the observer himself, or other persons besides those to whom the inference refers, are themselves capable of acting in the same way as the persons whose conduct is described. Hence arise constant intrusions of the observer's personality into calculations from which they should be rigidly excluded. The point may seem somewhat subtle, and I must therefore bespeak the reader's attention to the following remarks.

§ 10. The statistics with which we are concerned in Probability are composed, as already stated, in great part of the voluntary actions of men. They may relate, for example, to crimes, such as the frequently adduced instances of murders, thefts, and suicides; to what are intended to be virtuous actions, such as the sums annually expended in charitable or other such purposes; or to actions of an indifferent character, such as the number of marriages, or of insurances effected in the year. But of such portions of human conduct, as of most other portions, it is no matter of hypothesis, but a simple datum of experience, that in the long run, when we extend our observations over a sufficient space, a great and growing degree of uniformity is generally observable.

§ 11. Now between statistics of this kind and those which are concerned with what are not the immediate results of voluntary agency, whether the latter be of a purely involuntary character, as for example shipwrecks, or be results in which the human will is generally but a remote cause, as throws of dice, or births and deaths, there is one marked difference. It is this. We the observers, or any one else whom we suppose to occupy the position of observer, are ourselves beings like those whose conduct we tabulate and reason about, and the actions in question are such as we are or may be in the habit of performing ourselves. Hence it results that we are conceivably, if one may so say, a portion of our own statistics; we may suppose our own case to be included in the statistics under discussion. In many of the common examples taken from insurance, and above all from games of chance, the case is of course extremely different. There we may preserve with perfect consistency that purely Phenomenalist view in which we regard ourselves as looking passively on the successions of existences independent of ourselves. It would in fact be always difficult and often impossible not to take such a view there.

But though not impossible, it is exceedingly difficult to do the same when the things whose statistics we discuss are actions which men exactly like ourselves do perform, and which we any day may perform. To retain the correct view with rigid consistency it would indeed be necessary to exclude ourselves entirely from the statistics, in other words, to confine ourselves consistently to the observer's point of view, as we unavoidably do in the case of games of chance. We might help to compose the statistics of others, just as others compose the statistics for us, but we must not attempt to occupy both positions, those of observer and observed, simultaneously.

§ 12. It must be admitted, that owing to the peculiar character of the series of statistics of Probability, and the merely *average* truth with which we are there concerned, the inconsistent attempt just mentioned does not necessarily cause any error there. If indeed we were concerned with the absolute and universal statements of ordinary inference there would be error; the determination of a man, for example, to commit suicide when the inferential statement in which he was included had contemplated his abstaining from such an act, would falsify the inference. But no one man has power, by his own private conduct at least, to do any injury to an average. A registrar of deaths might drown himself as safely as any one else might, so far as affects the integrity of the formulæ with which he is concerned. His conduct is an isolated event, whereas the statistics continue indefinitely. Hence although his suicide is formally unwarranted, owing to the fact of its being an intrusion into statistics which had not contemplated it, it soon becomes overruled, and its effects, even had they ever been perceptible, gradually vanish from sight. In the long run such a disturbance of course could not show itself, whilst in the individual instance, by one of the fundamental hypotheses of Probability (the irregularity of the details), we should not be justified in taking notice of the disturbance.

§ 13. It is not therefore exactly by this stepping down of the observer into the arena of the statistics, unwarranted as it is, that the fallacy now to be noticed arises. It is rather by certain hypothetical intrusions to which the acknowledged practical harmlessness of the actual intrusion gives rise, that error and confusion are caused. Finding that any one observer may without mischief do very much as he likes amongst the statistics, similar invasions are conceived upon such a scale as to involve the destruction of the speculative or scientific view, and, as we shall presently see, to cause amongst other things the expression of a great deal of practical fatalism.

§ 14. A quotation from Buckle's *History of Civilization* (Vol. I. p. 25) will form a convenient introduction to the discussion now to be entered upon. After pointing out that among public and registered crimes there is none which seems so completely dependent on the individual, and so little liable to interruption as suicide, he proceeds as follows:—"These being the peculiarities of this singular crime, it is surely an astonishing fact, that all the evidence we possess respecting it points to one great conclusion, and can leave no doubt on our minds that suicide is merely the product of the general condition of society, and that the individual felon only carries into effect what is a necessary consequence of preceding circumstances. In a given state of society a certain number of persons must put an end to their own life[1]. This is the general law, and the special question as to who shall commit the crime depends of course upon special laws; which however, in their total action, must obey the large social law to which they are all subordinate. And the power of the larger law is so irresistible, that neither the love of life nor the fear of another world can avail anything towards even checking its operation."

§ 15. The above passage as it stands seems very absurd, and would I think, taken by itself, convey an extremely unfair opinion of its author's ability. But the views which it expresses are very prevalent, and are probably increasing with the spread of statistical information and study. They have moreover a still wider extension in the form of a vague sentiment than in that of a distinct doctrine. And as they are not likely to find a more intelligent and widely read expositor, or to be expressed in a more vigorous and outspoken way, I do not think I can do better than state my opinions in the form of a criticism upon this quotation.

---

[1] About 250 annually in London.

§ 16. One portion of the quotation is plain enough. It simply asserts a statistical fact of the kind already familiar to us, namely, that about 250 persons annually commit suicide in London. This is all that the statistics themselves establish. But, secondly, this datum of experience is extended by Induction. The inference is drawn that about the same number of persons will continue for the future to commit suicide. Now this, though not lying within the strict ground of the science of Probability, is nevertheless a perfectly legitimate employment of Induction. The conclusion may or may not be correct as a matter of fact, but there can be no question that we are at liberty to extend our inferences beyond the strict ground of experience, and that the rules of inductive philosophy will furnish us with many directions for that purpose. We may admit therefore that, for some time to come, the annual number of suicides will in all likelihood continue to be about 250.

§ 17. But it will not take much trouble to show that there is a serious fallacy involved in most cases in the expression of such sentiments as those quoted. I am anxious that it should be clearly understood that this fallacy finds no countenance in either of the two assumptions which are necessary for the establishment respectively of the rules of Probability and Induction, in those, namely, of statistical uniformity, and invariability of antecedence and sequence. In other words, the inference in the quotation would remain either unmeaning or false, in spite of our admitting that the number of persons who perform any assigned kind of action remains year by year about the same, and that the actions of each person are links in an invariable sequence [1].

§ 18. We have here forced upon our attention the distinction between the two standing-points which may be occupied when we are investigating human conduct. Let us briefly examine them in turn.

(1) There is, firstly, that speculative point of view which, as I have said, we are in consistency bound to retain. On this view all these assertions about the inutility of efforts and the inefficiency of motives are meaningless, or rather inappropriate. What we are then discussing is, not what people might do if they were to resolve to alter their conduct, but what we have reason to infer that they *will* do. We are not concerned with the results of hypothetical alterations—these results might be of extreme importance—but we are drawing inferences as to whether such alterations will be made. All therefore that can be established by the fact of the statistical results remaining nearly the same is, that the amount of the counteracting efforts and the strength of the antagonist motives remain the same, not that these efforts and motives are in any sense ineffective. To prove this last point it would be necessary to take very different ground, namely, to examine instances in which such efforts had been made

---

[1] It may prevent confusion if I remark here that my own opinion is in favour of Necessity, provided that nothing more is assumed in the meaning of that term than that where the antecedents are the same so will be the consequents. As stated in a former chapter, such a doctrine is necessary for the establishment of strict rules of Induction, though not for securing those of Probability.

and instances in which they had not, and to show that the results in each case were nearly or exactly the same.

§ 19. (2) In distinction from the above there is the view taken from the practical standing-point. Every agent, whether or not his conduct form part of any table of statistics, finds himself the centre of a sphere of action. This view receives immense extension by each person being able to put himself in imagination into the position of any other individual, or into that of any body of individuals. When this position is occupied the question becomes a very different one from that last considered. We are not now considering whether efforts *will* be made, but we are distinctly taking into discussion the different results according as they are made or not. This would be the most natural and appropriate explanation to be given of such remarks as those in the quotation before us, and could be the only one offered if we were referring to the efforts of a single individual or to those of a few people. All that any person could then mean by talking about the inutility of efforts would have no reference to the question whether efforts would really be made or not; he would simply mean that the difference, according as they were made or not, would be little or nothing.

It will scarcely be maintained, in this sense, that motives are feeble or efforts at suppression ineffective. Any considerable alteration in the belief of people as to a future world, or in their comfort in this world, would unquestionably have a great influence upon the number of murders or suicides. As regards the efforts of the clergy or magistrates to suppress the evil, however much these may be depreciated, it will not I apprehend be denied that something might be done towards *increasing* the annual number of those who destroy themselves,—by removing the police, for instance, from the neighbourhood of the Serpentine and Waterloo

Bridge. And it tells equally for our present argument, if it be admitted that the efforts of such persons could produce any consequence whatever, whether favourable or adverse.

§ 20. The reply to this would probably be, that though considerable consequences might really follow were we to suppose an alteration in the conduct of persons in authority, or in the belief of the people, yet that we have no right to introduce such an imaginary alteration, because we know that as a matter of fact it will not really take place. This is probably quite true, and I have no intention of denying it; but what it is requisite to draw attention to, and what seems to be often overlooked, is how in such a reply as this we may be shifting from one point of view to another. We are abandoning the view taken in the last section and falling back upon the speculative view. When the efforts of a few persons are contemplated, the hypothesis of their acting otherwise is admitted, but the consequent effect is pronounced to be insignificant, as might very likely be the case. When however the efforts of many are contemplated, the hypothesis of their acting otherwise and the consequent effect, which would then be great, are not admitted, on the plea that they are inconsistent with fact.

§ 21. Such a confusion as that discussed above may seem absurd, but I cannot help thinking that in this way considerable support is often given to that practical fatalism which expresses itself in the common complaints about the utter impotence of the individual, and the irresistible power of great social laws, and which shows itself in our conduct by a somewhat indolent and selfish disposition to let everything good or evil take its own course without troubling ourselves about it. It is observed that in the statistics of actions which may be the result, in their final form, of many different motives and of various conflicting struggles, there is yet year by year a marked uniformity. Instead of concluding from this, what alone ought to be concluded from the standing-point of a science of inference, that the motives and the efforts remain about the same year by year, a confusion is made between this and the practical view, and the doctrine is obtained that the efforts at repressing such conduct are unavailing.

§ 22. Such fatalistic views are often expressed in the form of disparaging comments upon the insignificance of individual efforts. In the sense in which this complaint is often made, I cannot but think that it is nothing more than an expression of our own indolence or selfishness, and really means, not that the results we could effect are small, but that we care little about them. Let us test this by taking some statistical uniformity, in which the motives that act to produce the result are almost entirely of a self-regarding character. Suppose that any one having ascertained that about a thousand persons, daily, in London, who could afford to eat a dinner, do for one reason or another go without it, were to announce this fact as a great social law of prodigious efficacy, and were to declare that its power was such, when examined on a large scale, that neither the fear of future hunger nor the love of good food could prevail towards even checking its operation, what would be the natural reply? The form of these statistics and the nature of the argument grounded upon them are pretty nearly identical with those of the example cited by Mr Buckle. The reply would probably be, that if we were professing simply to draw inferences, most of this talk about the impossibility of checking such actions was, to say the least, inappropriate; but that if we were taking the practical view, that is, if we were for any purpose putting ourselves into the position of one or more of the individuals in question, the statement was utterly false. Each one of those men could in most cases have eaten his dinner or not according as he pleased, and therefore the whole body could have done so under the same conditions. And no sophistry about free-will and necessity would be allowed for a moment to stand in the way of such a judgment. Equally absurd would it be considered to talk about the insignificance of individual efforts in the face of such a great social law. But were people perfectly unselfish, statistics of crimes would not, for the immediate purpose in hand, differ much from such an example as this. Almost any one individually might do something, and small bodies of men might do a great deal, towards diminishing crime, were it but in a single instance. When therefore it is vaguely complained that efforts are fruitless in the case of crimes, is there not some ground to think that the real meaning is that such efforts, on any much larger scale, are not likely to be made? And when it is urged that the individual can do nothing in this case, whilst no one would dream of making the same assertion in the other case, are we wrong in thinking that the real difference is that the attainment of one's own dinner is more universally regarded as a substantial good than the suppression or diminution of our neighbour's faults? I do not, of course, deny that we should find it much more easy to dissuade people from some courses of conduct than from others; all that is meant is, that there is no real distinction between the general deductions which may be drawn from one or another of these various kinds of statistical regularity.

§ 23. A few remarks, chiefly by way of summary, may be offered in conclusion, inadequate as they are for the discussion of so important a subject as that treated of in this chapter.

We have been engaged, throughout this Essay, in considering the laws of inference applicable to those classes of things which combine individual irregularity with aggregate regularity. Human actions of most kinds possess this property as unquestionably as do the throws of a die or the casualties caused by storms. The same laws of inference therefore which apply to the latter kind of events apply also to the former. But in saying this we are no more putting these two kinds of events upon the same footing than the historian is neglecting the distinction between virtue and vice when he employs the same rules of arithmetic to reckon up, say, the numbers who in any country have been burnt at the stake as martyrs and hanged on the gallows as thieves. To say that our attention should, for purposes of inference only, be fixed upon one quality in an event, does not imply that we are to forget the existence of other qualities in that event. When we come to examine the actions to which the statistics refer, we find of course that they possess many other properties besides that which makes them fitting subjects of Probability. Their consequences may be of overwhelming importance, they may be the result of long deliberation and of bitter mental conflict, they may be morally admirable or detestable. But with all these latter qualities the logician, as such, is not concerned. His task is to make inferences. He has to calculate the chance of an event, whether that event be the throw of a die, a shipwreck, or a suicide.

Select almost any kind of action, and we shall find, if we extend our observation over a sufficiently large body of men, that there is a uniformity in the performance of that action. I have already called attention to some erroneous inferences which are often drawn from the existence of such uniformity. It was pointed out in the last chapter, that causation, in the individual instances, could not easily be proved in this way. It has also been repeatedly insisted on that to generalize as to the continuance of such uniformities beyond the limits within which they have been actually observed, though in many cases perfectly legitimate, belongs to Induction and not to Probability.

But at present we are not concerned with such considerations as these. We will assume that there is a long-continued uniformity in the frequency of the performance of some action, against which, it may be, large classes of persons are struggling with their whole strength. What we are now concerned with is the vital importance of the distinction between what may be called the speculative and the practical views which we may take in reference to any such uniformity.

What we have to adhere to, in making inferences, is the speculative view. On this view we have no right, when talking about the future, to use any other expressions than those which denote simple futurity. To say that the agents 'must' perform certain actions, or 'cannot' perform others, is inadmissible. To say this is to fall into a fatalistic[1] fallacy, for it generally involves a confusion between certainty of inference on our own part and compulsion on the part of the agents.

But against fatalism, in anything which has a close connexion with our comfort or convenience, the ordinary European mind is protected by a healthy instinct of incredulity. We should try in vain, by any effort, to persuade people that each agent could not generally alter his conduct if he pleased, or, consequently, that any body of men could not produce an appreciable effect if they were to try. The plain man feels that such statements as these are absurd; the thinker knows that, whichever way the doctrine of Necessity be settled, that doctrine does in no way whatever come into contact with the practical problems of life when stated in the above form.

---

[1] By Fatalism I understand the doctrine that events really dependent in part upon human agency, will yet be equally brought to pass whether men try to oppose or to forward them. It is essentially distinct from Necessity, and is indeed rather entertained as a vague sentiment than as a definite doctrine. It is indeed difficult to state it with brevity without making it obviously involve a contradiction.

When however the efforts of the agent are directed towards securing, not his own good but the good of others, the promptings of our natural indolence and selfishness offer dangerous facilities for the reception of such fatalistic doctrines, at least in the minds of those who are only looking on at the struggle and not sharing in it themselves. It is one thing to entertain the conviction that certain results will remain for some time uniform, because the conflicting efforts which produce them remain uniform. It is quite another thing to conclude that the efforts on one side are themselves ineffective. But the mere spectator, if his sympathies are not active, finds it only too easy to step across the logical chasm which separates one of these opinions from the other. I cannot help thinking that much support is thus given to the doctrine which one hears uttered in so many different forms, and in every shade of dogmatism, by a certain school of writers, that the sorrows and the crimes of our fellow-men are only the necessary product of the existing state of society, and that the efforts of the individual are insignificant. There are many perhaps who would indolently tell some hard-working philanthropist that he could do nothing, who would yet be very much astonished if asked whether the trouble of their own doctor in coming to see them when they are suffering from any ailment produced insignificant results.

But the confusion between the speculative and the practical points of view produces, I think, still further consequences, quite as deplorable as those already described. Neither the Necessitarian nor the Fatalist need be men of blunted moral feelings. We might, that is, hate a wrong action though we thought it inevitable in the sense that the agent would not as a matter of fact choose to avoid committing it; we might even hate such an action though we thought it inevitable in the sense that the agent must do it whether he chose or not. But the complaint is often made, and I think not altogether unjustly, that the advocates of Sociology are too much in the habit of regarding crimes as being not only certain to happen, but as being morally indifferent. In so far as this complaint is true, I should think that such an apparent moral obtuseness of judgment (I shall not be misunderstood as hinting that this is accompanied by moral laxity in practice) is connected with that confusion between two distinct views which has occupied our attention during this chapter. The connection would be as follows. The speculative view is in one sense wider than the practical, for the former includes not only voluntary actions (the province of the practical view), but also actions which are not voluntary, as well as results which are not strictly speaking actions at all, such as the faces turned up by dice. In the great majority of subjects to which this view introduces us, moral praise and blame have no applicability. When therefore the two views are confused together, we are sometimes apt, not merely to hamper our practice by fatalism, but even to run the risk of debasing our moral judgment by regarding the actions of men with the indifference with which we regard the happening of things. It might thus result, for example, that we should not merely believe that the number of murders or suicides are so fixed that efforts are unavailing to counteract them, but even that we should feel little more affected at the commission of crimes than at the successions of the throws of a die.

Against every such confusion between the two views there is no safeguard comparable with that afforded by the habit of familiarizing ourselves with each of them. In other words, it might be advisable to temper one's speculations with a reasonable infusion of practice. Fatalism cannot easily exist in the fresh air of practical life. The hardest workers are generally the most hopeful men, and in unselfish efforts will be found the best corrective to that depression which is apt to be produced by a too exclusive devotion to the speculative view. We should thus avoid the danger of always discussing the joys and the sorrows of our fellow-men in a way which, though legitimate when we are avowedly taking a partial view of the subject, too easily lapses into indifference or cynicism if we suffer ourselves to forget how partial that view is.

INDEX.

Agencies in Probability, 50
— their analysis, 53
— natural, 58
Anticipation, rules of, 221
Arbuthnott, 245
Aristotle, 310, 322
Arrangement of stars, 251
Assumptions, arbitrary, 164
Averages, 90
— and means, 94
— use of, 97, 349
— truth, 434, 452

Babbage, 369
Bain, 179
Balls, drawing of, 438, 453
Belief, emotion of, 138
— gradations of, 101
— growth of, 178
— half, 116
— justification of, 122, 134, 147
— measure of, 112, 129
— standard of, 327
— zero point of, 378
Bentham, 321, 326
Bernoulli, 85, 409
Births, male and female, 84, 244
Boole, 177
Bravais, 349
Buckle, 235, 241, 473

Binomial → exponential 33

Buffon, 135, 186, 379, 409
Burgersdyck, 314
Butler, 190, 283, 360, 393, 460

Carlisle tables, 151
Causation, chain of, 228
— definition of, 225
— need of, 193
— use of, 187
— and chance, 236, 457
— and design, 224, 459
— and miracles, 455
Chance and design, 242
Classes, reference to, 194, 418
Classification, 258
Combinations, 81, 93
Communism, 400
Conceptualism, 256, 279
— objections to, 263
Contest of improbabilities, 444
Cournot, 250
Crackanthorpe, 314, 323
Cramer, 409
Crofton, 55
Curve of Error, 28, 40, 338, 353, 356

Deductive proof in Probability, 82
De Morgan, 77, 104, 114, 118, 159, 161, 180, 235, 291, 299, 311, 377, 404

H.Kimpton 15sh.–
$2.60

Todhunter, 169, 441
Tontines, 405
Tucker, 109
Types, existence of, 37
  — heterogeneous, 39
  — human, 42
  — variable, 13, 43, 87

Ueberweg, 313, 320
Uniformity, meaning of, 233
  — of nature, 266

Value of expectation, 124
Value of life, 210
Volition, human, 26, 60, 79, 248

Walford, 400
Wallis, 314
Whately, 260, 300
Whewell, 97, 217
Whist, 69, 426
Whitworth, 166, 413
Witnesses, combination of, 452
  — truthfulness of, 422, 433
Wolf, 312

CAMBRIDGE: PRINTED BY C. J. CLAY, M.A. AT THE UNIVERSITY PRESS.

LANE MEDICAL LIBRARY

This book should be returned on or before
the date last stamped below.

MAR 1 5 1972

175
46
376

Venn, J.
The logic of chance
80724

NAME	DATE DUE
ML Wall (Derm)	MAR 15 1972